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Sovereign Reason

AND OTHER STUDIES IN THE PHILOSOPHY OF SCIENCE

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Contents

Introduction

THE SIXTEEN ESSAYS in this collection were written for different occasions, and have been previously published. They are reprinted here with only minor editorial changes. They do not constitute a systematic discussion of a single philosophic problem, nor do they exhibit a consecutive development of a single philosophic theme.

This collection of papers nevertheless does deal with a set of related problems. All save one of the essays are critical studies of contemporary philosophers who have been occupied, in one way or another, with the content or method of modern science, and who have thereby sought to illumine the nature and operations of human reason. These essays thus constitute an evaluation of various proposals by influential writers for resolving many paramount issues in the philosophy of science. Whatever unity the contemporary philosophy of science may have in respect to the subject matters and the problems falling into its province, I can also claim for this book. The unity is clearly not a tightly knit one. It is indeed possible to subsume the problems treated by writers on the philosophy of

science under four diverse though related types. The studies in this collection touch upon all four types, since each thinker discussed has concerned himself with one or more of them. The reader may find helpful the following brief description of the major divisions into which contemporary philosophic commentary on science can be conveniently classified.

The relation of theory to gross experience—of "abstract reason" to the "nature" encountered in ordinary perception and overt action—is a theme that is as old as western thought. Philosophers have discussed it in technical language, and poets have sung variations on it.

Do not all charms fly
At the mere touch of cold philosophy?
There was an awful rainbow once in heaven.
We know her woof, her texture; she is given
In the dull catalogue of common things.
Philosophy will clip an Angel's wings,
Conquer all mysteries by rule and line,
Empty the haunted air, and gnomed mine—
Unweave a rainbow, as it erewhile made
The tender-person'd Lamia melt into a shade.

It is a theme covering problems which became acute with the development of mathematical physics three centuries ago; and the character of current theories in the natural sciences, possessing contents that are apparently quite "alien" to the manifest features of experience, has augmented its importance. It is a theme to which contemporary philosophers of science have devoted much attention, so that a good fraction of recent philosophic literature is by explicit design a critique of scientific "abstractions." A well-nigh continuous spectrum of diverse proposals is available for making explicit the ways in which theoretical concepts and theoretical objects in the sciences are related to the materials of familiar experience. The general problem these proposals seek to solve is perhaps the most persistent and central philosophic issue under current discussion.

The nature and basis of reliable knowledge is the theme of a second group of problems. Genuine knowledge was once widely identified with absolutely certain knowledge, and this identification continues to be made by many contemporary thinkers. Some of them claim to achieve such knowledge by way of immediate sen-

sation, others by way of private intellectual "insight," still others by way of a special mode of reasoning. On the other hand, there are also many thinkers who do not equate knowledge with infallibility; and by these the conclusions of the sciences—usually, though not invariably, the conclusions of the physical sciences—are often taken to be the supreme examples of warranted belief. Which if any of these competing claims has merit, what the grounds are for the alleged reliability of scientific knowledge, how probable inferences are to be interpreted and "justified," and what are the principles underlying the valid estimation of evidence, are focal questions in contemporary philosophy of science.

A third group of problems is generated by the perennial quest for a total view of the universe. In the opinion of many men, the various special sciences are incapable of dealing with "ultimate" questions, so that answers to these must be sought elsewhere:

> Beyond the bright searchlights of science,
> Out of sight of the windows of sense,
> Old riddles still bid us defiance
> Old questions of Why and Whence.

Some philosophers have sought to resolve such riddles by constructing speculative schemes of cosmic evolution which purport to "explain" the emergence and destiny of every occurrence in nature on the basis of primordial principles of unilinear development. Other writers have tried to master such riddles by advancing some "higher synthesis" of the detailed findings of the special sciences, so that the restricted theories of the latter are the supposed consequences of an inclusive "philosophic" theory about the foundations of the universe. On the other hand, many philosophers have rejected as misplaced such efforts at obtaining a comprehensive view of things. Some of them, however, have adopted what to them appears as the sound method for achieving this objective, by devising systems of interrelated categorial distinctions that ostensibly formulate the "pervasive traits" of existence. Such systems are alleged to have the value of a connected and complete "map" of the universe, so that every character displayed in nature must necessarily illustrate one of the categories in the system. Accordingly, such categories are often claimed to formulate the ultimate principles of intelligibility—the basic intellectual tools for criticizing and evaluating the

conclusions of specialized scientific inquiries. It will be evident that most of the issues of traditional metaphysics find their place in this third group of questions discussed in current philosophies of science.

Finally, there is a class of problems dealing with the relations of science and society. Some of these problems are concerned with the social determinants and the social consequences of the scientific enterprise; others are concerned with the bearings of scientific findings on systems of individual and social value. Many questions in this group are of a forthright empirical character, and those who seek to resolve them must abide—as they frequently do—by the canons of historical and sociological investigation. Other questions, however, are not so easily resolved even in principle, since their very formulation often depends on antecedent commitments on issues falling into the three previously mentioned groups of problems. At one extreme are those writers who approach these questions from the perspective of the dictum: "Science carries us into zones of speculation, where there is no habitable city for the life of man." At the other extreme are commentators on human affairs who see in the patterns of activity which physical science finds pervasive throughout nature, the true models for the organization of human society. In any event, in no area of the philosophy of science are the issues so complex and so heatedly debated as in this one.

The major themes listed in this catalogue are recurrent in the history of thought, and are all important. They enter into all reflective thought or action which is controlled by some conception of the character of human reason and by some notions concerning the place of man in nature. They are themes, moreover, that are intimately related to the actual practice of science; and scientists have frequently found it imperative, for the sake of advancing their own work, to clarify some of the issues involved in them. The need for considering broad methodological and substantive questions is especially acute in periods of scientific development, such as ours is, in which revolutionary changes in theoretical outlook are taking place. Such developments tend to upset deeply rooted beliefs concerning the structure of the physical universe as well as those concerning human behavior; and those developments provoke, in consequence, a reexamination of the basis of cognitive certitude and of the accepted criteria of intelligibility. The themes

cited are accordingly not of exclusive concern to professional philosophers. Philosophically minded scientists as well as professional philosophers have contributed to their analysis.

There are, however, ways of conceiving and attacking problems associated with those themes that are sterile and unilluminating, just as there are approaches to theme which are fruitful and clarifying. Much contemporary philosophy of science does not merit these latter characterizations, for it does not always satisfy the minimal conditions that philosophic analysis must meet if it is to be informative and illuminating. No question in the philosophy of science is significant, if its formulation is so general that specific evidence drawn from identifiable subject matter is never relevant to the evaluation of any proposed answer to the question. No discussion is pertinent to a problem, if the discussion is conducted out of all relations with the context that fixes the sense of the problem. No mode of attack on a problem is fruitful which generates for every puzzle it pretends to solve a fresh difficulty that is a duplicate of the original one. And no proposed answer to a problem is clarifying, if it assumes an absolutely completed body of knowledge as a condition for effectively resolving the question. Conformity to these requirements is undoubtedly not a sufficient condition for valuable work in the philosophy of science, but failure to conform to them seriously qualifies the merits of any analysis in this domain of philosophy.

A few examples of doubtfully valuable contributions to the philosophy of science, which illustrate each of the major themes cited above, will give point to these dicta. A number of techniques have been proposed for exhibiting the relation of the abstractions of science—such as "points" and "straight lines" in geometry, or "instants" and "atoms" in physics—to the material of crude experience. Proposals like those of Russell and Whitehead, for example, that ignore the concrete ways in which scientific abstractions are employed in various contexts of inquiry, and that substitute for one set of theoretical notions another set of equally remote formal constructions, throw little if any light on scientific procedure. Such proposals may be important contributions to some branch of formal study, and are undoubtedly fascinating exhibitions of intellectual virtuosity. They do not provide helpful answers to the difficulties that initiated the analysis.

Secondly, it is significant to ask, for example, why under specified conditions men make incorrect perceptual judgments or draw unwarranted conclusions. Such questions are in principle resolvable, for they are concerned with the identification and the elimination of sources of possible error in determinate contexts. But there is no *general* "problem of error" in the pursuit of knowledge. It is not rewarding to pursue limited questions like the one just cited, if inquiry into them is based on a radical scepticism concerning the very possibility of ever making correct judgments. It is not fruitful to do this, or to raise in wholesale fashion a "problem" about the "existence of an external world"—as Reichenbach, Russell, and others have in effect done—if only because no identifiable context of inquiry can be specified in which such "problems" can be coherently formulated.

Thirdly, the quest for a comprehensive view of things can be the pursuit of an intelligible and worthy ideal. It is an ideal to which various special departments of study are progressively contributing. It is an ideal whose realization can perhaps never be more than approximate, though even partial realizations of it, especially when embodied in great literature, may serve as general directives for human effort. But when the quest for a total view of nature is so conducted that it can terminate only in a set of "ultimate" categories which are compatible with *every possible* order of events, or which ascribe by vague analogy to everything in nature traits that are *known* to characterize only a limited sector of it, a misleading verbal game has been substituted for a search for genuine knowledge. In any event, the various schemes of basic categories that have been constructed—whether by Peirce or by Whitehead or by Dewey—which supposedly codify the "generic" or "irreducible" traits of existence, have still to prove their worth as alleged instruments either of explanation, criticism, or fruitful classification.

Finally, the task of making explicit the import of scientific discoveries for human values is a perennially important one. But that task is radically misconceived when standards of human excellence are alleged to be derived, or derivable, from the characters which physical science ascribes to its postulated sub-microscopic objects. For the context in which the predications of sub-atomic physics are significant, is not the context in which judgments concerning

human values are relevant. No amount of dialectical prestidigitation can convert the presumably necessary conditions for the existence of everything whatsoever, into the sufficient conditions for the manifestation of special human virtues.

In their commentaries on the nature and import of science, practicing scientists as well as professional philosophers have thus frequently obscured rather than illumined fundamental issues. It is not altogether anomalous that scientists are not uniformly the best interpreters of their procedures and their theoretical discoveries. What is loosely called "scientific method" is usually a habit of workmanship acquired by engaging in successful inquiry, rather than a codified set of principles to which scientists explicitly subscribe. In fact, most workers in specialized branches of research rarely give serious attention to methodological problems that do not contribute directly to the solution of concrete research tasks. The philosophy of science which practicing scientists profess on ceremonial and other occasions when they discuss the broader significance of their enterprise and their achievements, is often but an echo of philosophical ideas uncritically acquired in their youth. It is nonetheless the case that some of the most penetrating and clarifying analyses of the operations of scientific intelligence and of the broad significance of theoretical constructions, have been the contributions of philosophically minded workers in special branches of science. The writings of such men as Helmholtz, Mach, Hertz, Duhem and Poincaré, to mention only a few of the illustrious dead, provide ample evidence for this judgment.

Professional philosophers writing on science are not exempt from the danger of becoming unwitting captives of an uncritically accepted philosophy. Philosophers are usually familiar, however, with a variety of intellectual traditions, and they presumably have some training in the criticism and the logical analysis of broad issues. This danger is in consequence perhaps smaller in their case than it is in the case of most specialists in the sciences. On the other hand, philosophers suffer from the serious disadvantage that they frequently possess no first-hand familiarity with the actual operations of scientific inquiry or with the actual functions of scientific abstractions. They therefore run the grave risk of propounding issues and proposing solutions for them which have no conceivable bearing on any context or phase of science. Distance from an object may make

more of it visible. But when the distance is sufficiently great, the object may disappear in a haze, and an alleged description of it may then be the description of a fog. Moreover, without the restraining control of a clear subject matter, there is often an irresistable temptation to "generalize" a problem whose sense is fixed by a definite context, to a point where the terms of the generalized formulation no longer posses differential meanings. The "solutions" which are then advanced for such "generalized" questions will often be functions of the particular technique of analysis that happens to be in favor; but such "solutions" will also be irrelevant to anything that is of serious concern in the conduct of inquiry. The dangers and pitfalls here briefly listed are of course not insurmountable; and the writings of Peirce, Dewey, Reichenbach, Whitehead and Russell show, despite their various limitations, how much the philosophy of science owes to the analyses of professional philosophers.

The philosophy of science is a difficult branch of intellectual analysis. It requires for its effective cultivation a rare combination of solidly founded substantive knowledge, analytical and constructive skill, and—not least—a sharp sense for relevance. Even those who possess these traits in abundance do not always exhibit them to best advantage, as the essays in this collection try to show. There are, indeed, no uniform standards of competent workmanship which control analyses in the philosophy of science, and which are also binding upon those who attempt to evaluate the analyses of others. But the best work in this domain of philosophy, so it seems to me, has been done by men who have sought to understand human reason by examining its operations in controlled inquiry, and who have interpreted the meaning of theoretical constructions in terms of their manifest functions in identifiable contexts I would like to believe that the perspective of such a contextualistic analysis also controls the critiques of contemporary philosophers of science contained in this collection of essays.

1

Malicious Philosophies of Science

THERE IS NO SUBSTANTIAL EVIDENCE for the widely held view that changes in the content and standards of theoretical inquiry are uniquely determined by changes in the economic and political structure of society. To be sure, scientific inquiries are often initiated and subsidized by those concerned with problems of commerce and technology, and the manner in which scientific discoveries are assimiliated by a society depends on its economic and political organization. But once a department of inquiry establishes its traditions of workmanship, so the history of science seems to indicate, the course of subsequent developments in it is determined by the materials explored, by the talents and skills available, and by the logic of theoretical investigation.

In almost every age, however, the attitudes which men assume toward personal and social issues have been often justified by them in terms of their understanding of the methods and latest findings of science. Professional scientists have frequently used their special-

ized knowledge to buttress or criticize the institutions of their day; but publicists, religious leaders, and philosophers have usually played a more prominent role in this task of evaluating the general social import of scientific methods and scientific theories. Such evaluations do not, in most cases, flow from the specific character of scientific methods or their technical achievements; they issue from the social and religious commitments of those who make them, and are symptomatic of the stresses and strains in the social scene.

In the midst of actual and impending disaster, men are inclined to listen to any voice speaking with sufficient authority; and during periods of social crisis, when rational methods of inquiry supply no immediate solutions for pressing problems, spokesmen for institutional and philosophic theologies find a ready audience for a systematic disparagement of the achievements of empirical science. Ideas which the advance of knowledge had partially driven underground during periods of fair social weather, are then insolently proclaimed as panaceas for public and private ills. The assured methods of scientific control and understanding, because they effect no wholesale resolution of problems and because they yield no conclusions beyond the possibility of error and correction, are then declared to be unsuitable guides for rational living.

The mounting economic and political tensions of our own age have not failed to produce a literature of this type. From various quarters—from men of science, historians of ideas, as well as outspoken representatives of theological systems—there has come a flood of criticism of modern science and of the secular naturalism which has accompanied its growth. The criticism has been neither uniform nor consistent. But the common objective of much of it has been the limitation of the authority of science, and the institution of methods other than those of controlled experimentation for discovering the natures and values of things. Many recent evaluations of science have thus had an obviously malicious intent; and the present essay will seek to determine briefly to what extent, in the case of several influential types of philosophies of science, good sense has been sacrificed to such malice.

1.

Undoubtedly the most solidly intrenched intellectual basis for the current disparagement of science is a well-known but nonetheless

questionable theory of knowledge upon which experimental method is made to rest. This type of critique starts with the familiar fact that in many theories of the natural sciences, especially physics, the various sensory qualities (such as colors and sounds) receive no *explicit* mention, and that it is only the quantifiable traits of things (such as mass and length) which are noticed. The immediate conclusion which is then drawn is that sensory qualities are not properties of objects in their own right, but are dependent on the activity of an *immaterial mind*. The remainder of the argument may then proceed along either of two lines of interpretation. According to one of them, more common in earlier centuries than in our own, the traits studied by the natural sciences are the only genuinely real things, while the directly experienced qualities are only a passing appearance. The sights and sounds and smells of the human scene are thus taken to have only a "subjective" existence and to be the otiose by-products of the true executive order of nature. According to the second interpretation, currently highly fashionable, the qualities apprehended in daily experience are the concrete and exclusive reality. It is these qualities which are held to constitute the intrinsic natures of things; and since these qualities are allegedly psychic products, it is they which are regarded as the intelligible substance of the world. In putting to one side the qualitative character of existence the natural sciences are consequently preoccupied with shadowy abstractions, which have at best only a mean practical value; and the laws which are the outcome of scientific inquiry, far from expressing the true nature of things, fail to grasp and convey the dynamic reality of existence.

On either interpretation, therefore, the world is split into two discontinuous realms. One of them, the proper domain of natural science, is a "mysterious universe" forever foreign and essentially unintelligible to the common experience of mankind; the other, the locus of enjoyments and values, is the theater of the mind's activities and creations, and is the only reality in which mind can feel confidently at home. On either alternative, the human scene is endowed with character so distinctive that the procedures of the empirical sciences can provide no guide to it. For the controlled methods of experimentation are held to be relevant only to the realm of abstract quantity, so that the entire field of valuation, of deliberation and moral choice, is exempted from the norms of experimental inquiry.

Qualitative reality, which by hypothesis has an inherent connection with mind and consciousness, must therefore be explored by techniques different from those employed in the positive sciences; and in this realm, claims to truth must be subjected to canons of a radically different kind. Imagination, intuition, introspection, and modes of emotional experience, are some of the ways which have been recommended for grasping genuine reality and for understanding human affairs.[1]

Nevertheless, the actual character of scientific method offers warrant neither for such attempts to limit its authority, nor for the radical dualism of the qualitative and the quantitative, the mental and the physical, upon which those attempts thrive. A brief mention of some obvious features of experimental procedure will be sufficient to show how inadequate are the analyses on whose basis the authority of scientific method is impugned.

In the first place, however "abstract" scientific theories may be, those theories can be neither understood nor used except in contexts of familiar qualitative discriminations. These contexts are tacitly taken for granted in the explicit formulation of theories, and are neither ignored nor contemned by the practicing scientists. Consider, for example, some of the operations involved in even so elementary a process as the measurement of spatial magnitude: standards of magnitude must be constructed, requiring the use of familiar

1. Two citations from recent writers will help convey the flavor of the alternative methods which have been proposed.

"Imagination is more adequate to reality than reason, for reality is not rational; therefore poetry and religion are better adapted to the real than the sciences. The real is not abstract and general. It is always concrete and individual; that is the reason why imagination alone can grasp it, whereas the intellect cannot fully conceive it. A theory of imagination . . . is urgently needed as a foundation for ethics, esthetics, philosophy of history and of religion, and even for metaphysics." (Richard Kroner, in a paper read before the Third Conference on Science, Philosophy and Religion, as quoted by *The New York Times*, August 29, 1942.)

". . . It is an axiom of sound method that any experience is, in some manner and to some degree, *intrinsically* cognitive. An experience of love . . . is at the same time an insight into the loveable nature of what is loved; an experience of moral urgency . . . is an insight into the rightness of the action to be performed; an experience of reverence . . . is an insight into the divinity of what is reverenced. Every such experience is a growth in wisdom and the wisdom is not testable by scientific techniques. . . ." (Philip Wheelwright, "Religion and Social Grammar," *The Kenyon Review*, vol. 4 (1942), pp. 203-204.)

bodies of daily experience; the relative constancy of the standards must be established, thus necessitating the noting of qualitative changes such as temperature; and the mutual relations of bodies must be discriminated, thus involving the identification and distinction of bodies on the basis of such qualities as color and the texture of surfaces. In general, no metaphysical opposition between the qualitative and the quantitative is forced upon us in this process, since the institution of quantitative standards is simply the ordering and the discrimination of qualitative continua. The view that the subject-matter and the data of the physicist are opaque pointer-readings is clearly a falsification of the scientist's procedure—a falsification which becomes more evident the more thorough is our examination of the full spectrum of scientific operations.

In the second place, there can be no doubt that the colors, sounds, and other characteristics we perceive in our every-day affairs owe their existence not only to the objects they are commonly believed to qualify, but also to complicated mechanisms (including physiological ones) of which common-sense is frequently unaware. But it does not follow that these qualities are therefore constituted out of some "mental stuff," or that a "mind" (in the sense of a disembodied, experimentally unidentifiable agent) is required for bringing them into existence. Whatever the conditions may be for the occurrence of colors, for example, experience shows extended *surfaces* to be colored; and if it is held that colors are "mental," traits (such as that of being extended) must be attributed to the mind which are the presumptive distinguishing marks of physical objects. In that case, however, what becomes of the notion of mind as a disembodied entity? But dialectic aside, there is no shred of evidence that in addition to complicated physico-organic processes any other "agents" are required to produce the qualitative manifolds of experience. The postulation of an additional agent (held to be something distinctively "mental") is on par with the caprice of endowing the planets with souls in order to account for their motions. The actual procedures of the natural sciences thus offer no ground for the alleged dualism between the mental and the physical; and accordingly, even the semblance of a reason disappears for limiting the scope of experimental techniques.

And in the third place, the "abstractness" with which natural science is charged as a fatal weakness, is in fact a trait of all cogni-

tion. All cognition involves the making of distinctions and the recognition of some things as relevant and others as not; in this sense, therefore, all cognition abstracts from its subject-matter and prescinds those features from it which bear on the problems at issue. To *know* the course of the planets is not to engage in periodic journeys around the sun; to *know* the factors and conditions of a human transaction is not the same as to participate in its joys and sorrows. More generally, it is not the function of knowledge to reproduce its own subject-matter; and to refuse to make abstractions of any sort is to abandon knowledge in favor of uninformed feeling and blind experience. Accordingly, just what is the quarrel of those critics who find fault with the abstractness of science? Do they seriously claim that the theories of science are not relevant to their subject-matters? Do they maintain that there are ways essentially different from and superior to those employed in the natural science for ascertaining the conditions for the occurrence of things and events? Or do they disdain science simply because it does not supply what no knowledge worthy of the name can offer—an unanalyzed reduplication of its own subject-matter? In either event, their discontent flows from a willful romanticism and a disregard of the historical achievements of the natural sciences; it provides no valid ground for excluding the operations of experimental inquiry from the domain of human affairs.

2.

A second widespread critique of scientific methods is at bottom a variant of the one already considered. It does not *explicitly* disown the authority of science in human affairs; but it does recommend the adoption of such vague and irresponsible canons of experimental control that it in effect argues for the exclusion of the logical methods employed in the positive sciences from the study of social problems. This view rests its case on two major claims; that in the past the natural sciences have mistakenly tried to "reduce" all features of the world to "mechanical" or "materialistic" properties; and that recent advances in our knowledge have demonstrated the breakdown of the "mechanical" categories of classical science. The present view, like the previous one, maintains that the human scene is so discontinuous with the "lower levels" of nature that a common logic of inquiry cannot be adequate to all

of them; it therefore concludes that problems affecting human destiny must be investigated on the basis of canons of validity and intelligibility which differ radically from those used in the natural sciences.

Let us examine these contentions. And first, what are we to understand by the terms "mechanical" and "materialistic"? When practicing physicists characterize an explanation as "mechanical," they mean a theory which, like the one developed by Galileo and Newton, explains a class of changes *entirely* on the basis of the masses and the spatial and temporal relations of bodies. In this quite precise sense of the word, Maxwell's electro-magnetic theory is not a mechanical theory, and with its advent in the 19th century the earlier hope that the science of mechanics would become the universal science of nature was gradually abandoned. But those who accuse classical science of being "mechanical" are anything but so definite as to the real point of their charge. For according to them the Darwinian theory of organic evolution as well as Maxwell's theory of electro-magnetism are mechanical, although neither of these theories satisfies the physicist's definition of "mechanical." The only clear meaning which can be given to the accusation is that classical science is *deterministic*—in the sense that it attempts to discover the precise conditions for the occurrence of phenomena, without benefit of final causes and without invoking experimentally unidentifiable causal agents.

But if this is the meaning of the charge, the claim that modern science no longer operates with mechanical categories is singularly ill-founded. As already noted, even classical physics recognized that mechanical theories (in the technical sense of the word) are not universally adequate; and recent researches into atomic phenomena have only fortified this conclusion. There is, however, nothing in modern research which requires the abandonment of the *generic* ideal of classical science: to find the determining conditions for the occurrence of phenomena, expressible in terms capable of overt empirical control. Thus, even modern quantum-theory—although it employs technical modes of specifying the character of physical systems which are different from those used in classical mechanics—is deterministic or mechanical (in the loose sense) in so far as it rigorously specifies the unique physical conditions under which certain types of changes will occur. Similarly, modern genetics is

no less deterministic than the Darwinian theory, since the former even more completely than the latter has succeeded in disclosing the mechanisms or structures involved in the transmission of characteristics from one generation to another. It is therefore simply not true that recent advances in knowledge have demonstrated the untenability of the logical canons of classical science.

The claim that the world-picture according to natural science "reduces" everything to blind, undifferentiated collocations of material particles, and thus fails to do justice to the distinctive traits of human behavior, likewise rests on a misconception as to what the sciences in fact accomplish. Consider, for example, the following comments of the late Lord Balfour:

> What are we to say about a universe reduced without remainder to collections of electric charges radiating throughout a hypothetical ether? . . . We can certainly act on our environment, and as certainly our actions can never be adequately explained in terms of entities which neither think, nor feel, nor purpose, nor know. It constitutes a spiritual invasion of the physical world.—it is a miracle. . . . We are spiritual beings, and must take account of spiritual values. The story of a man is something more than a mere continuation of the story of matter. It is different in kind. . . .[2]

That this represents a caricature of what the achievements of physics imply, will be evident if we recall that the sciences seek to determine the precise *conditions* under which events come into being and continue to exist. For in ascertaining those conditions the sciences do not *thereby deny* the existence of any traits found in nature, whether in the human scene or elsewhere. In particular, physics has assumed the task of finding the most general and pervasive constituents and circumstances of existing things; it does not legislate away as unreal or non-existent—and could not do so without contradiction—the things and events into whose conditions of existence it inquires. The explanations which physics offers for the traits and changes it studies, consist of careful specifications of the conditions under which those traits and changes occur; and no other sense of "explanation" is relevant in discussing its findings. Whether these explanations can be stated entirely in terms of a special class of entities and their relations (for example, in terms of the distribution of electrically charged particles), is a specific

2. Arthur J. Balfour, in an essay contributed to *Science, Religion and Reality* (edited by Joseph Needham), pp. 15-17.

empirical issue which can be resolved only by detailed empirical inquiry; it cannot be settled by dialectic, or by an *apriori* fiat such as that the living cannot be explained in terms of non-living.

Criticisms of natural science such as the following are therefore altogether pointless, since they operate with mythological conceptions as to the character of its explanations:

> With the faintest and simplest element of consciousness, natural science meets something for which it has no pigeon-hole anywhere in its system. . . . Mind at its best is autonomous. Granting that it is connected mysteriously and intimately with physical processes that natural science claims as its own, it cannot be reduced to those processes, nor can it be explained by the laws of those processes.[3]

Explanations of "mind" in terms of physical processes do not wipe out the distinction between the behavior of inorganic masses and the distinctive activities of men; nor do they pretend to deduce somehow the direction of those activities from physical laws containing no mention of purposive behavior. Such explanations simply state the generalized conditions for the occurrence of "mind." Accordingly, the only form of "reduction" with which the natural sciences may rightly be charged, is the form which consists in ascertaining the structures under which specific traits are manifested; and it is clear that if those sciences failed in effecting such a reduction, they would fail in achieving the objective of knowledge. The conclusion seems unavoidable that those who would exclude the logical methods of the natural sciences from fields of social inquiry, on the score that these methods commit their users to the "reductive fallacy," are in effect recommending the abandonment of the quest for the causal determinants of human affairs.

One final claim, associated with the charge that the logic of natural science is "reductive," remains to be considered—the claim that human traits are "emergent properties" on a "higher level" of existence than are those with which physics and chemistry are concerned, so that the methods employed by these disciplines cannot be adequate to the study of the higher emergent qualities. Some examples will make clear the chief features of the theory of emergence. However much we may know about the interaction of hydrogen and oxygen with *other* elements, so it is said, it is

3. Brand Blanshard, "Fact, Value, and Science," in *Science and Man* (edited by Ruth Nanda Anshen), pp. 189, 203.

impossible to infer from such knowledge the fact that they combine to form water, and in particular, the qualities which emerge when water is formed could never be predicted from those data. Similarly, no amount of knowledge concerning the physics and physiology of the human body makes it possible to deduce the "spiritual" characteristics of the organism as a thinking, purposive creature. Nature is thus conceived as a system of levels of emergence, each level requiring a peculiar mode of study; and *a fortiori*, the distinctive qualities of human beings can be satisfactorily explored and understood only when inquiry into them is conducted on the basis of a logic specific to "spiritual" subject-matter.

But the following brief remarks will be sufficient to blunt whatever force the argument from the alleged facts of emergence may be supposed to have. It is indeed not possible to *deduce* the properties of water (for example, its transparency or its ability to quench thirst) from those of hydrogen and oxygen, *if* the former properties do not enter into the premises from which the deduction is attempted. For in general, no statement containing a given term "P" can be deduced from a class of statements unless the latter also contain that term. In one sense, therefore, the main contention of emergent evolution is simply a logical truism. In the second place, although the occurrence of certain traits may be left unexplained by one theory, a different theory (perhaps a revised form of the first one) may supply a satisfactory explanation. For example, the theories of physics which were accepted at the beginning of the 19th century were unable to account for any chemical facts, although present-day physics is in the position to explain the occurrence of many chemical reactions. Accordingly, whether a quality is to be regarded as an "emergent" or not is relative to a specific theory, and is not an *inherent* fact about that quality. It also follows that since no theory of science can be regarded as necessarily final, traits which at one time are taken to fall into the province of one specialized discipline, may at some later date be explained on the basis of theories developed in a different branch of science. This has certainly been the history of such sciences as chemistry, biology, and even psychology, in their relation to physics. And finally, if the doctrine of emergence is seen in this light, no clear reasons remain why the logic of experimental inquiry—as conducted in the natural sciences—has no authority over investigations into human

affairs. Indeed, as the natural sciences have become more comprehensive they have provided an enriched understanding of human traits. No theoretical limits can be set to such a progressively widening scope of the sciences of nature. And what is no less to the point, these fresh achievements have involved no surrender, on the part of the natural sciences, of the procedural principles under whose guidance they obtained their historical successes.

3.

The views which have been noticed thus far attempt to limit the scope of scientific methods on the basis of considerations that are at least nominally scientific in character. The criticisms of science to which attention must next be directed do not even pretend to adduce scientific grounds for their claims, and are frankly based upon explicit theological and metaphysical commitments for which no experimental evidence is invoked. The chief burden of their complaints is that science offers no "ultimate explanation" for the facts of existence; and their chief recommendation is the cultivation of "ontological wisdom" as the sole method for making "ultimately intelligible" both the order of the cosmos and the nature of the good life.

Some citations from recent writers will exhibit more clearly than would a paraphrase the unique mixture of pontifical dogmatism, oracular wisdom, and condescending obscurantism which seems to be the indispensible intellectual apparatus of this school of criticism. Professor Gilson characterizes the plight of science as follows:

> This world of ours is a world of change; physics, chemistry, biology, can teach us the laws according to which change actually happens to it; what these sciences cannot teach us is why this world, taken together with its laws, its order, and its intelligibility is, or exists. . . . Scientists never ask themselves *why* things happen, but *how* they happen. Now as soon as you substitute the positivist's notion of relation for the metaphysical notion of cause, you at once lose all right to wonder *why* things are, and why they are what they are. . . . Why anything at all is, or exists, science knows not, precisely because it cannot even ask the question. To this supreme question the only answer is that each and every particular existential energy, and each and every particular existing thing depends for its existence upon a pure Act of existence. In order to be the ultimate answer to all existential problems, this supreme cause has to be absolute existence. Being absolute, such a cause is self-sufficient; if it creates, its creative act must be free. Since it creates not only being

but order, it must be something which at least eminently contains the only principle of order known to us in experience, namely, thought.[4]

And Professor Maritain, building on the alleged subordination of science to metaphysics, indicates some of the immediate consequences of this hierarchial arrangement:

Science . . . is distinguished from wisdom in this, that science aims at the detail of some special field of knowing and deals with the secondary, proximate or apparent causes, while wisdom aims at some universal knowing and deals with prime and deepest causes, with the highest sources of being. . . . Wisdom is not only distinct from but also superior to science, in the sense that its object is more universal and more deeply immersed in the mystery of things, and in the sense that the function of defending the first principles of knowledge and of discovering the fundamental structure and organization thereof belongs to wisdom, not to science. . . . Science puts means in man's hands, and teaches men how to apply these means for the happiest outcome, not for him who acts, but for the work to be done. Wisdom deals with ends in man's heart, and teaches man how to use means and apply science for the real goodness and happiness of him who acts, of the person himself. . . . Science is like art in this that though both are good in themselves man can put them to bad uses and bad purposes: while in so far as man uses wisdom . . . he can only use it for good purposes.

The paleontologist does not step out of his sphere when he establishes the hypothesis of evolution and applies it to the origin of the human being. But the philosopher must warn him that he is out of his field when he tries to deny for that reason that the human soul is a spiritual soul which cannot emanate from matter, so that if once upon a time the

4. Etienne Gilson, *God and Philosophy*, pp. 72, 140. Although Whitehead's manner of arriving at his speculative cosmology is radically different from that cultivated by neo-Thomists, his evaluations of the limitations of natural science are frequently not dissimilar. He comments as follows on "the grand doctrine of Nature as a self-sufficient, meaningless complex of facts": "Newton left for empirical investigation the determination of the particular stresses now existing. In this determination he made a magnificent beginning by isolating the stresses indicated by his law of gravitation. But he left no hint, *why in the nature of things there should be any stresses at all.* The arbitrary motion of the bodies were thus explained by the arbitrary stresses between material bodies, conjoined with their spatiality, their mass, and their initial states of motion. By introducing stresses—in particular the law of gravitation—instead of the welter of detailed transformations of motion, he greatly increased the systematic aspect of nature. But he left all the factors of the system—more particularly, mass and stress—in the position of detached facts *devoid of reason* for their compresence. He thus illuminated a great philosophic truth, that a dead nature can give no reasons. All the ultimate reasons are in terms of aim at value. A dead nature aims at nothing." A. N. Whitehead, *Modes of Thought*, pp. 183-4, italics not in the text.

human organism was produced by a mutation of an animal organism, it was because of the infusion of a soul created by God.[5]

Although criticism of a position is futile when those who hold it make a virtue of its mysteries and when they regard themselves as superior to the usual canons of scientific intelligibility, those who are not so fortunately placed may find the following observations not irrelevant. In the first place, there is a perfectly clear sense in which science does supply answers as to "why" things happen and are what they are. Thus, if we ask why the moon becomes eclipsed at certain times, the answer is that at those times the moon moves into the earth's shadow; if we ask why the moon behaves in this way, the answer is given in part by the theory of gravitation; if we ask why bodies behave in the manner predicated by this theory, the answer is supplied by the general theory of relativity. On the other hand, if we repeat this question concerning relativity theory, no further answer is at present forthcoming, so that for the present at least this theory is an "ultimate" or "brute" fact. Furthermore, if some day relativity theory should become absorbed into a unified field-theory embracing both macroscopic and microscopic phenomena, the unified field-theory would explain why the equations of relativity theory hold, but at the same time it would become the (perhaps only temporary) "ultimate" structural fact. In science the answer to the question "why" is therefore always a theory, from which the specific fact at issue may be deduced when suitable initial conditions are introduced. The point of these familiar remarks is that no matter how far the question "why" is pressed—and it may be pressed indefinitely—it must terminate in a theory which is itself not logically demonstrable. For no theory which explains why things happen as they do and not otherwise can be a logically necessary truth. It follows that those who seek to discern the laws of nature to be necessary, as well as those who "hope to see that it is necessary that there should be an order of nature," are violating an elementary canon of discoursive thought.

In the second place, it is obvious that anyone who invokes an

5. The first paragraph is taken from the essay "Science and Wisdom," contained in *Science and Man*, pp. 66-7, 72, 94. The second paragraph is from the essay "Science, Philosophy and Faith," contained in the volume *Science, Philosophy and Religion*, the proceedings of the First Conference on Science, Philosophy and Religion, p. 181.

"absolute cause" (or God) to explain "why" the world exists, merely postpones settling his accounts with the logic of his question: for the Being who has been postulated as the Creator of the world is simply one more being into the reasons of whose existence it is possible to inquire. If those who invoke such a Being declare that such questions about His existence are not legitimate, they surmount a difficulty only by dogmatically cutting-short a discussion when the intellectual current runs against them. If, on the other hand, the question is answered with the assertion that God is his own cause, the question is resolved only by falling back upon another mystery; and at best, such a "reason" is simply an unclear statement of the grounds upon which scientists regard as unintelligible the *initial* "why" as to the world's existence. But a mystery is no answer if the question to which it is a reply has a definite meaning; and in the end, nothing is gained in the way of intellectual illumination when the discussion terminates in such a manner.

In the third place, the postulation of an "absolute cause" or an "ultimate reason" for the world and its structure provides no answer to any *specific* question which may be asked concerning any particular objects or events in the world. On the contrary, no matter what the world were like, no matter what the course of events might be, the same Ultimate Cause is offered as an "explanation." This is admitted in so many words by Professor Gilson:

> The existence or non-existence of God . . . is a proposition whose negation or affirmation determines no change whatever in the structure of our scientific explanation of the world and is wholly independent of the contents of science as such. Supposing, for instance, there be design in the world, the existence of God cannot be posited as a *scientific* explanation for the presence of design in the world; it is a *metaphysical* one.[6]

But just what does an "explanation" explain when it explains nothing in particular? What understanding of our world does a metaphysics provide which is compatible both with a design in the processes of nature as well as with its absence, with the existence of specific goods as well as with their non-existence, with one pervasive pattern of causal interactions as well as with another?

6. *Op. cit.*, p. 141.

A high price in unintelligibility must be paid when the canons of scientific discourse and inquiry are abandoned.

And finally, the assumption that there is a superior and more direct way of grasping the secrets of the universe than the painfully slow road of science has been so repeatedly shown to be a romantic illusion, that only those who are unable to profit from the history of the human intellect can seriously maintain it. Certainly, whatever enlightenment we possess about ourselves and the world has been achieved only after the illusion of a "metaphysical wisdom" superior to "mere science" had been abandoned. The methods of science do not guarantee that its conclusions are final and incorrigible by further inquiry, but it is by dropping the pretense of a spurious finality and recognizing the fallibility of its self-corrective procedures that science has won its victories. It may be a comfort to some to learn that in so far as man uses "wisdom" he can aim only at the good; since the most diverse kinds of action —kindly as well as brutal, beneficent as well as costly in human life —are undertaken in the name of wisdom, such a testimonial will doubtless enable everyone engaged in such an undertaking to redouble his zeal without counting the costs. But it is not wisdom but a mark of immaturity to recommend that we simply examine our hearts if we wish to discover the good life; for it is just because men rely so completely and unreflectively on their intuitive insights and passionate impulses that needless sufferings and conflicts occur among them. The point is clear: claims as to what is required by wisdom need to be adjudicated if such claims are to be warranted; and accordingly, objective methods must be instituted, on the basis of which the conditions, the consequences, and the mutual compatibility of different course of action may be established. But if such methods are introduced, we leave the miasmal swamps of supra-scientific wisdom, and are brought back again to the firm soil of scientific knowledge.

4.

The final variety of current criticism of science to be considered rests its case on the alleged facts of history. The development of science, it is admitted, has brought with it an increase in material power, a broadening of the average span of human life, and a wider distribution of innumerable goods than was possible in earlier days.

Nevertheless, so the criticism runs, human happiness has not increased and the quality of life has not improved. On the contrary, increased power over material nature has generated a deadening monotony and uniformity in men's lives, has produced ghastly brutalities, cataclysmic wars, and fierce superstitions, and has undermined personal and social security. Science deals with instrumentalities and is incapable of determining values; and with the spread of secular naturalism and the consequent decline of religious influences, men have grown insensitive to the distinction between good and evil, and have identified material success with ethical excellence. Intellectual historians join hands with preachers and publicists in placing the blame for contemporary Fascism upon the demoralizing effects of positivistic philosophy. And in language solemn and threatening Professor Maritain warns his readers of the dreadful consequences which allegedly flow from scientific naturalism:

> Let us not delude ourselves; an education in which the sciences of phenomena and the corresponding techniques take precedence over philosophical and theological knowledge is already, potentially, a Fascist education; an education in which biology, hygiene and eugenics provide the supreme criteria of morality is already, potentially, a Fascist education.[7]

Whatever may be the validity of the causal imputations contained in such criticism, it cannot be denied, unfortunately, that many of its characterizations of modern society are well founded; it is certainly not the intent of the following comments to dispute them. It must nevertheless be noted that the implied judgment, according to which the quality of modern life is inferior to that of earlier societies unburdened by an institutionalized natural science, is based on a definite set of preferences or values in terms of which human history is surveyed. But while it is clear that there is nothing reprehensible in employing definite standards of valuation (for exam-

7. In the essay "Science, Philosophy and Faith," *op. cit.*, p. 182. Neo-Thomists have no monopoly in the making of such casual imputations. Thus, in his essay "Fact and Value in Social Science," Professor Frank Knight writes as follows: "In the field of social policy, the pernicious notion of instrumentalism, resting on the claim or assumption of a parallelism between social and natural science is actually one of the most serious of the sources of danger which threaten destruction to the values of what we have called civilization. . . . It is a serious reflection that the unsatisfactory state of affairs in social science has largely resulted from the very progress of science. . . ." *Science and Man* (edited by Ruth Nanda Anshen), pp. 325-6.

ble, such as are involved in Catholic Christianity), such standards need to be made explicit and should not be assumed as self-evident and above criticism. For it is sheer dogmatism to assume that only one conception of spiritual excellence is valid; and it is the height of discourtesy and parochialism to damn a society as immoral simply because its standards of excellence differ from one's own. Moreover, comparative judgments as to the happiness of men are notoriously untrustworthy, unless they are based upon objective measures of well-being. And if the material conditions of life are discounted as indications of "true" happiness, the critic's evaluations of different cultures are a better guide to his own preferences and loyalties than to the ostensible subject-matter of his judgment.

Let us turn to the causal imputations contained in the criticism under consideration. Almost no argument is required to show that if the growth of science may validly be held responsible for the ills of modern society, then the fact that men marry may no less validly be declared the cause of the evils of divorce. For surely, divorce would be impossible unless men first married, just as our present social distresses would not exist unless the advance of scientific knowledge had first made possible our present institutional structures. But to convert marriage into the cause of divorce, and the advance of secular knowledge into the cause of social ills, is to convert the *context* in which problems arise into an *agent* responsible for our inability to master them. As well argue that in order to eliminate the evils of divorce men must stop getting married, as recommend the de-secularization of modern society as a solution for its difficulties; in either case the conversion of context into cause is an unintelligent performance. The development of science has brought with it new opportunities for the exercise of human energies, and has helped set the stage for the emergence of new problems. How many of these problems have remained unsolved because vested interests and the cake of custom have prevented the application to them of the methods of controlled inquiry which the natural sciences use so successfully, it is difficult to judge. But in any event, the indictment of scientific intelligence as solely responsible for our present difficulties not only involves an arbitrary selection of one factor from a complex of others distinguishable in the social scene; it arbitrarily rejects the one instrument from which a resolution of these difficulties may reasonably be expected.

Consider, finally, the charge that science "cannot determine values," and that therefore the apprehension of the elements of a good life must be obtained through some form of emotional experience. Now whatever be one's views as to the nature of values—whether they are regarded as relative or absolute, dependent on human preferences or not—it must be admitted that a science (such as astronomy) which does not concern itself with values and which does not contain value-terms in its vocabulary, is incompetent to establish value judgments. The thesis that *some* sciences cannot determine values is thus trivially true. On the other hand, every rational appraisal of values must take cognizance of the findings of the natural and social sciences; for if the existential conditions and consequences of the realization of values are not noted, acceptance of a scheme of values is a species of undisciplined romanticism. Accordingly, unless values are to be affirmed on the basis of uncontrolled intuition and impulse, all the elements of scientific analysis—observation, imaginative reconstruction, dialectic elaboration of hypotheses, and experimental verification—must be employed. Knowledge of biology and hygiene are indeed not sufficient for an adequate conception of the moral life; but if one may judge from the historical functions of some philosophic and theologic ideas in perpetuating economic inequality and human slavery, and in sanctioning the brutal shedding of human blood, neither is a knowledge of philosophy and theology.

It is often urged that what is good for man lies outside the province of scientific method, because the determination of human goods requires a sympathetic understanding of the human heart and a sensitive, individualized perception of the qualities of human personality; and the exercise of such powers, it is maintained, has no place in the procedures of science. But this objection rests, at bottom, on a failure to distinguish between the psychological and sociological conditions under which ideas originate, and the validity of those ideas. Thus, it is reported of Schiller that he used to place a rotting apple on his desk for the stimulus the odor of the fruit provided to his writing; but while this is an interesting item about the conditions under which Schiller obtained his inspirations, it has no bearing on the issue as to quality of his poetry. Similarly, the unusual circumstances—whether personal or social—under which many seers and religious prophets obtained their visions are not

relevant in a consideration of the soundness of their moral exhortations. More generally, the psychology and the sociology of research are not identical with its logic. Those who disparage the application of scientific methods to the evaluation of human goods, on the ground that those methods exclude the exercise of a sympathetic imagination, are not only mistaken in their factual allegations; they are also well on the road to identifying the sheer vividness and the emotional overtones of ideas with their validity.

2

Recent Philosophies of Science

Although the name "philosophy of science" is a recent one, the types of inquiry which it covers are not. Speculation on the meaning and conditions of our knowledge of the world is as old as systematic inquiry, and accordingly the first writings on the philosophy of science are those of the ancient Greeks. Indeed, the works of Plato and Aristotle are the beginning of that long series of commentaries upon the content, the nature, and the limitations of science which are the characteristic products of the history of philosophy itself. It would be an anomaly if an institution with consequences as important for human weal and woe as western science has had, should not constantly receive interpretations of its significance. And it is surely no accident that some of the most vital periods of philosophic activity coincide with periods of "revolutionary" scientific changes.

It is in large measure because we are living in a period of such changes in science—particularly in physics, logic, and mathematics

—that the philosophy of science has been receiving the attention of philosophically minded scientists as well as of professional philosophers. The process of adjusting and reorganizing older conceptions in the light of newer ones is a perpetual feature of reflective inquiry, and the task has traditionally been assumed by philosophy.

However, it is not alone the sheer novelty of recent findings which has brought the philosophy of science into prominence. One of the arresting features in the history of science is that, although, in terms of the control it provides and the contributions it makes to daily living, science has come ever closer to men's bosoms, the theoretical foundations of the special sciences have become progressively less familiar and their accounts of the ways of the world have become more puzzling. The assurance frequently given to the perplexed beneficiary of science that its theoretical foundations are simply "organized common-sense" carries little conviction or enlightenment, if that organized common-sense outrages every familiar belief he holds. Confusion, not clarity, emerges from tales of expanding spaces, waves of probability, particles with velocity but no position, or the determination of present activities by configurations existing at some future time. In spite of its practical successes modern science thus seems to cast a veil over the moving forces of nature; and the familiar world of sights, sounds and smells, of substantial objects, solid bodies, and stable conditions of behavior, appears to be cut off from the world reported by science.

The status of the abstractly formulated objects and relations of science, and their mode of connection with the gross and familiar objects and relations of daily experience, are therefore the deepest concern of contemporary philosophies of science. No writer falling under the loose rubric "philosopher of science" completely neglects the task—designated by Whitehead as the distinctly philosophic one—of being a "critic of abstractions," and of attempting to assess the relations of the unobservable and abstract entities of science to the qualitative and concrete environment. Philosophers of science differ profoundly in their modes of approach and in their conclusions; they differ less strikingly in the general problem to which they set themselves.

It is not feasible within a limited space to discuss the different philosophies of science currently proposed. Instead, a few influential types will be distinguished and the characteristic views of their

representatives will be outlined. Moreover, while there are analyses of science which base themselves on the actual or supposed findings of biology or upon the allegedly distinctive character of the historical and social sciences, it is the philosophies of science whose point of departure is the physical and mathematical sciences which, in recent years and for obvious reasons, have received the major share of attention. The present survey will in the main confine itself to these, and four fairly distinct approaches to the interpretation of science will be considered. They are as follows.

(1) The approach seeking to identify categorial traits in immediate experience which are believed to be pervasive traits of everything real and which may therefore be discovered in the objects disclosed by the sciences. In terms of these pervasive traits continuities are claimed to be established between the remote and the near, between entities not accessible to direct observation and those which are, between the abstractions of science and the concrete objects of familiar experience. The outstanding writer who approaches the materials of the theoretical sciences in this vein is unquestionably Whitehead.

(2) The approach which emphasizes the rich content of experience in domains other than theoretical physics as against the "abstract ballet" of theoretical formulations, and which accepts as final the apparent incompatibility of the findings of everyday experience with those of mathematical physics. It nevertheless erects a bridge between these "two worlds": it offers a theory as to the way in which mathematical physics is constructed, and concludes that its man-made but opaque constructions hide a reality similar to that which we find in ourselves. The best-known protagonist of this view is Eddington.

(3) The approach which accepts theoretical science simply as an instrument of control, and for which all problems concerning the meaning of theoretical science are resolved either in terms of its social usefulness or in terms of the principles of dialectic materialsm. This approach is frequently, though not necessarily, coupled with a historical thesis concerning the major forces making for progress in science.

(4) Finally, the approach which seeks to fix the meaning of scientific statements by examining the explicit rules or habits governing their usage. It insists that scientific conclusions cannot be

understood unless the procedures involved in testing and applying them are taken into account, and it maintains that clarity of understanding can be won only by noting the specific functions within inquiry of theories and concepts. This approach fosters the critical study of scientific language and behavior. It is an approach which frequently goes under the somewhat unprecise and misleading name of "operationalism."

I.

Relativity and quantum theory have been the occasion for a thoroughgoing criticism of the ontology alleged to be at the basis of classical physics. This ontology, called "scientific materialism," derives its strength from the techniques and concepts employed in the mechanics developed by Newton and his successors. In that system, according to Whitehead's interpretation, space and time are simply vast receptacles filled with senseless, purposeless matter. Bits of matter are assumed to have sharply confined properties, and the full natures of material objects can therefore be specified by their spatio-temporal positions, independently of any essential relations to other regions and other times. In consequence, matter "just does what it does do, following a fixed routine imposed by external relations which do not spring from the nature of its being."

The classical device of isolating certain features of bodies for the purpose of theoretical analysis is legitimate; but it becomes an instance of the fallacy of "misplaced concreteness," and more particularly of the fallacy of "simple location," when those features are assumed to be exhaustive of the concrete nature of things. It is a fallacy, according to Whitehead, because no concretely existing things possess observable features with simple location or without essential relations to the remote occurrences in the full universe. It is because of the commission of this fallacy, he believes, that the Nature celebrated by poetry and religion was converted by mechanistic science into a scene of purposeless activity, where stars and molecules and human beings "blindly run," and where colors and sounds and smells are no longer traits of things but are properties of percipient minds. Nature was bifurcated into a qualitative world apprehended in perception and a world causing those qualities, and no intelligible connection was supplied between these heterogeneous realities. But in truth there are no actual points with dimensions, no

lines without breadth, no particles of matter with sharply confined regions of influence or non-relational properties. Whitehead's first task is to explain how such neat abstractions are derived from the immediately apprehended features of the concrete world. His second task is to formulate a system of categories and principles, adequate to the world of direct experience as well as to the remote regions of the world explored by theoretical physics.

The first task is discharged by him through the device baptized as the "principle of extensive abstraction," which has been hailed as the "prolegomena to every future philosophy of nature." Its main idea can be briefly illustrated. We have no direct experience of dimensionless mathematical points, and no reason to believe that any bit of reality is such a point. However, we do have direct experience of one box or volume extending over another. Imagine a series of Russian eggs in which each egg encloses another; we must, however, suppose that there are an infinite number of eggs in the set, in order that there should be no smallest egg which would enclose no other egg. As we follow the series of converging volumes we approach something that seems like a point. There is, however, no limiting volume in the set. Instead of postulating a dimensionless point as the limit, for whose existence there is no evidence whatsoever, Whitehead proposes to take the infinite series of convergent volumes as itself constituting a point. It turns out that points so defined have all the requisite mathematical properties, while at the same time they are exhibited as entities standing in complex relations to all bodies and not as simple substances isolated from everything else. In analogous ways, by specifying appropriate "abstractive routes," Whitehead defines other configurations and abstract notions studied in physics. In this way, it is claimed, we may bridge the gaps between the concrete facts of sense and the refined concepts of science.

The primary object of Whitehead's cosmological scheme is to prevent the bifurcation of nature into an accessible and a transcendental reality, by opposing to the philosophy of scientific materialism a philosophy of organism. His central assumption is that the "immediate occasions" accessible to direct inspection (e.g., the self-knowledge we have of our body's activity), must display the categories and the structures of activity fundamental to all of nature. The most concrete things are identified as *events*, and their inherent

character is to exhibit transition or passage into other events. But this passage is not a linear succession of discrete and independent events, since however narrowly an event may be specified further determinations are presupposed: every event extends over other events and is in turn included in still others. Every emergent event intrinsically involves some aspect of all things, since it mirrors within its own limited being the larger field in which it finds itself. An event, like a Leibnizian monad, is full with the past and big with the future. Thus, the event consisting of the apprehension of a landscape does not involve a castle, a field, a cloud, simply in themselves; that event is constituted by the perception of castle, field, and cloud "over there" from the standpoint of the perceptual unification "over here."

Nature is thus a structure of perspectival unifications and evolving processes. The stable objects of science and familiar experience are recurrent patterns of aspects within these unifications or events. These "enduring objects," whether they be electrons, atoms, mountains, or animals, are taken by Whitehead to be "organisms" of various degrees of complexity. They have "life-histories," they are partially formed by the "inheritance" of aspects from their own past, they have "memories" of earlier segments of their life-histories, they are capable of "envisaging" possibilities as yet unrealized, and they form "societies" with powers of "creative adaptation" to their environment. The human organism is itself a society, and the paradigm of its structure is found everywhere in nature. Every feature in this cosmology thus corresponds to some factor in what is alleged to be our immediate experience. Its theology as well as further details and refinements must, however, be passed over.

Whitehead's philosophy of science is so full of acute insights and is supported by so much expert knowledge of the details of theoretical science, that only the blind and the foolhardy may seem capable of rejecting it. But however that may be, two fundamental objections to it must be briefly stated.

(a) The principle of extensive abstraction succeeds in doing none of the things that may reasonably be expected from an instrument devised for the criticism of abstraction. It is a mathematical calculus whose application to the matters at hand raises the very problems it was intended to solve. Thus, for mathematical purposes, a point may be defined as an infinite set of overlapping volumes. But no

empirical subject-matter involves *infinite* sets of volumes, and no experiment could decide whether something alleged to be a point is indeed a point if the relations between an infinite set of objects would first have to be determined. Whitehead's principle does not therefore explain how any of the concepts of science, frequently defined in terms of infinite series or limiting processes, are connected with and applied to finite subject-matter.

(b) The attempt to understand the esoteric objects of science in terms of the categories of direct experience suffers from at least one fatal defect. These categories possess only such meaning as the context of overt human behavior provides; accordingly, their extension to new contexts having different operative qualities involves serious ambiguities and distortions of words. Is it in any way illuminating to characterize molecules as societies of atoms, possessing memories, and with powers of choice and creative adaptation? The "society" of atoms is different from any type of human society, their capacity for "memory," "choice," and "creative adaptation" is admittedly unlike the similarly named powers of human beings; ruling out these differences converts atoms into human beings or, conversely, the latter into the former. The use of like-sounding labels for distinct things serves to create only the appearance of systematic knowledge and clear understanding, and invites confusion by wrenching words from their anchorage in determinate and recognizable meanings.

2.

Perhaps the most widely read contemporary writer on the general significance of science is Eddington. Who does not know his famous account of the two tables, one of them the commonplace article of furniture, the other the scientific object—the former extended, comparatively permanent, colored, solid and substantial, the latter mostly emptiness and a collection of insubstantial electric charges rushing about with great speed? Who has not heard his story of the unfortunate elephant sliding down a grassy hillside which, in the hands of the physicist, all become dissolved into bundles of pointer-readings?

These bright images do not tell the whole story of Eddington's philosophy; and although the whole story cannot here be told, the general outline of its plot can be explained. Four leit-motifs are fundamental to it. In the first place, Eddington claims that physical

theory studies the linkage of pointer-readings, and that these linkages hang together in an endless cycle, with no intrinsic reference to the actual world. According to him, the definitions of physics have a circular character, each being specified in terms of others, but without revealing what substantial material they stand for. Thus electric force is said to be defined as something which causes the motion of an electric charge, while an electric charge is something which exerts electric force; accordingly, "an electric charge is something that exerts something that produces motion of something that exerts something that produces . . ."

Somewhere, however, a reference to what is actual must be introduced; for example, a certain set of pointer-readings must be identified with the color red. And according to Eddington, it is in *consciousness* that contact with actuality is made. For all our knowledge of the environing world comes to us in the form of messages transmitted along the nerve-fibres to the seat of consciousness, and it is only "our own ends of the fibres that we actually know." Everything else is reconstructed by inference from the contents of our consciousness. Accordingly, the material of the actuality with which direct contact is made is of mental character. This is the second leit-motif.

Science tells us nothing about the intrinsic natures of atoms or elephants or anything else, since for it they are simply schedules of pointer-readings. The world of science is thus a "shadow world," shadowing forth an "external world" whose nature is inscrutable for physics. Since, however, the sole point at which contact is made between theory and actuality reveals the latter to be of mental stuff, why not accept this as an indication that the inscrutable substance is also mental? This is indeed Eddington's conclusion. The stuff of the world is mind-stuff, and the symbolic world of science has for its substratum something akin to thought, something in which our own mental consciousness lies, and something in which we may find a Universal Mind.

The fourth theme concerns the logical status of such laws of nature as the law of gravitation and the laws of conversion of mass and energy. These laws *seem* to formulate objective structures of the world. But on Eddington's view, the permanence and invariance which we seem to note in the flux of existence are not objective traits of the latter; they are evidence for the mind's

demand for permanence, for the fact that the processes of nature have been fitted into frames of law which are of the mind's own choosing. Eddington thus follows Kant in all essentials if not in details. He attempts to deduce the fundamental principles and constants of physics from *a priori* assumptions concerning the mind's powers, and thereby claims to show that not only is the *stuff* of existence spiritual but that its basic *laws* also spring from the mind's activity.

The mainstays of Eddington's argument are well-worn articles of furniture in the mansions of traditional philosophy. His subjectivism, his religious pan-psychism, his apriorism, are familiar to readers of Berkeley, Lotze and Kant. It is impossible to state in short compass the serious difficulties which these positions have to face, and present comment must be reserved for raising briefly just one issue. How relevant as critiques of scientific abstractions are Eddington's writings? The answer here suggested is that they are of negligible relevance, because he fails in the main to examine the concepts and techniques of physics in terms of their recognized meanings and functions, and because his argument thrives on using terms in one context in a way which is intelligible only in a very different one.

Two examples must suffice to make clear as well as to support this charge. Every set of pointer-readings (e.g., the mass of an elephant) represents distinctions in qualities and behaviors with respect to which laboratory instruments have been previously calibrated; the numbers are theoretically significant only in so far as they are obtained in definite ways from definite material. The process of measurement is therefore not confined to reading off the coincidences of pointers with marks on a scale. Consequently, in no identifiable sense is the world of physics an opaque, shadowy world. Again, Eddington contrasts the "common-sense table" with a "scientific table," and implies that because the former is "solid" while the latter is "mostly emptiness" the common-sense and the scientific views are incongruous. But the expressions "solid" and "mostly emptiness" are defined or specified in just those contexts in which it is appropriate to predicate the former of tables and not the latter. If critical tests show that a given table is solid, the analysis of the table into a structure of electrons is valid only if that structure exhibits the property in question; and for that structure

of electrons the characterization "mostly emptiness" has no assigned meaning. It is therefore difficult to escape the conviction that the foundations of Eddington's philosophy of science consist of outrageous puns.

3.

The third type of philosophy of science will require only brief exposition. It is professed, among others, by a number of scientists in this country and England, whose object is to show the social implications of theoretical research and thereby to rob modern theories of their air of mystery. They explain, frequently with admirable lucidity, the new sources of power opened up by science, the economies in human effort which it is capable of effecting, and the close interdependence of science and the technological arts. The dominant emphasis is thus placed upon scientific theories as engineers' "blue-prints for action," rather than as disclosures of some final reality.

There might be little to object to this approach to science if those who take it would not confuse their thesis by other claims: that scientific research *ought* always to be conducted with the social good in view, or that all significant research is *in fact* determined by the organization and practical needs of society. There is, however, no evidence for the proposition that a warm social conscience is the best guide for the conduct of research; there is no ground for assuming that everyone knows what is socially desirable, that what is not of immediate social value will never prove to be so, or that devotion to socially valuable results necessarily contributes to the fullest development of systematic knowledge. There is overwhelming evidence to show that the content and direction of theoretical research is in the main controlled by technical problems internal to the sciences, and that changes in the social economy do not in general constitute the sufficient conditions for the specific content of theoretical inquiries.

A number of writers in this group make the further claim that the methods as well as the conclusions of science conform to the triad of "dialectical principles" sacred to many followers of Hegel and Marx. For example, Levy has argued that the occurrence of a distinct type of wave produced by a ship after it reaches a critical velocity illustrates the law of abrupt change of quality

with sufficient increase of quantity; and Haldane has borne witness to the inestimable value of all three laws in his own biological researches. Space is lacking to examine these and other claims for the virtues of the dialectic laws. It must suffice to recall Einstein's remark that philosophers are fortunate indeed in being always able to establish the agreement of the latest findings of science with their own metaphysical principles. The principles of dialectic can be guaranteed never the fail, because their terms have no fixed content; they can be stretched to cover any facts whatsoever, after the facts have been independently established. Were it not for the mysterious profundity claimed for them, and did they not serve to check inquiry by the glib finality with which they resolve all problems, the laws of dialectic could be left unreproached as a curious verbal game. As instruments for disentangling the web of theoretical constructions, however, they are as barren as the purest of vestal virgins.

4.

No single writer can be taken as an adequate representative of the fourth approach to the philosophy of science, for this approach does not aim at a compact system but at painstaking analyses of the details of scientific procedure. A few of the more prominent names associated with it must, however, be mentioned: Mach, Poincaré, Duhem, Campbell, and Peirce, for their analyses of the structure of physical theories and definitions; Russell, Wittgenstein, Tarski, and Carnap, for their contributions to the logic and methodology of the formal sciences; James, Dewey, Cohen, Neurath, and Woodger, for their critiques of abstractions in biology, psychology, and social theory. But this brief roster suggests only faintly the wide range of specific analyses which have been made.

The writers who fall into this group do not form a distinctive "philosophical school"; there are among them realists, pragmatists, positivists, Kantians, and solipsists. None the less, there are some common principles of analysis implicit in their contributions—even though, unfortunately, they do not always conform to them. In the first place, there is a distrust of wholesale philosophizing which lives upon isolating conclusions from the procedures warranting them, and which fails to note either the hypothetical char-

acter of scientific constructions or the distinct logical functions these have in determinate situations. Accordingly, effort is concentrated upon studying the overt and symbolic operations integral to scientific research, and upon analysis of particular theories and concepts. Stress is also placed on the facts that the conclusions of science are not necessarily final, that science develops by a self-corrective method, and that it does not rest upon self-evident truths.

Secondly, there is a striving after precision of statement, and a keen awareness of the intellectual traps which language may create for the unwary. With the rejection of the doctrine that clarity is a matter of apprehending simple ideas or essences, the attempt is made to discover the meaning of terms by study of the overt uses to which they are subject. Much labor thus goes into formulating and making explicit the rules of usage or the habits which govern the meaning of terms. The speculative, constructive character of scientific theories is recognized, and effort is therefore made to discriminate those features in them which are determined by stubborn fact from those which serve simply as a scaffolding for the scientist's activity.

And thirdly, scientific constructions are often critically evaluated without legislating to the scientist the methods or the limits of his specific enterprise. For it is often possible, in terms of canons of evidence implicit in the traditions of science, to exhibit flaws and unsupported claims in scientific constructions, or to point out the distinctive logical features and advantages of alternative conceptions. On the other hand, the history of the so-called "logic of discovery" shows the futility of seeking prescriptions for obtaining success in research. Writers in this group are often concerned with the logical evaluation of scientific formulations, but not with canons for discovering them.

A few simple illustrations will perhaps convey the spirit of this approach to the philosophy of science.

(a) It has been said, even by eminent physicists, that though we may know much about what energy *does*, we do not know what it *is*. But to say that we know what energy does, means that the equations of physics containing this term are supported by adequate evidence and that the circumstances under which those equations may be applied are fairly well established. Since the mean-

ing of the term "energy" in a theoretical science is determined by the system of equations into which it enters and by the rules governing their usage, the word hides no mystery; we do in fact know what energy "really is" if these equations and rules are known. The puzzles generated by recent theories according to which certain particles of matter may have determinate velocities but not simultaneously determinate positions, can be removed in an analogous way. For in this context the phrase "particle of matter" is employed according to rules which differ from the rules governing this phrase in other more familiar contexts. Accordingly, we must not associate the usual images with the phrase, so that the alleged incongruity of the physical world with the world of experience turns out to be simply a difference in the way like-sounding phrases are employed in different situations. But this is a familiar phenomenon, and we have learned long ago that the word "work" can have different applications in mechanics from those it has in economics, without either of these disciplines being therefore incoherent or unintelligible.

(b) Legal and political theory is a fertile field for the conversion of modes of behavior into substantial agencies; and the reification of terms like "property," "contract," "sovereignty," "equality," and "freedom," have often stood in the way of effective social reform. Thus a problem frequently discussed is the nature of "corporate personality." Legally incorporated groups can conduct their affairs with advantages lacking to men not so organized, but for many purposes corporations have the legal status of "persons." They are, however, a curious kind of persons, for they cannot be imprisoned or die natural deaths; and they may transact business in one place though legally they have their locus in another. Accordingly, the nature of corporate personality has puzzled many jurists, and they have argued that corporations have the status of spiritual beings—disembodied and yet efficacious, self-subsistent but with inherent rights of individual men. Such a hypostasis is unnecessary, however, if the significance of the attribution of "personality" to corporations is examined operationally. For a corporation is precisely that kind of "person" which the law of the land specifies: its nature is completely exhausted by the permitted ways or possibilities of action compatible with the legal system in force. There is thus no more mystery in the fact that a corporation has

its legal focus in one place while it acts in another, than in the fact that a bell is located in one room and is heard in another—both facts can be converted into insoluble puzzles, if the modes of behavior are first excluded from the "natures" of things considered and then conceived as additional substances. The concepts of the social sciences thus lend themselves to precisely the same sort of analysis as those of the physical sciences; they are intelligible in terms of the rules and habits implicit in the applications which are made of them.

It would be pleasant to report that those who profess to follow this approach to the philosophy of science always conform to its ideals. This is, regrettably, not possible. Careless analyses, loose formulations, hasty generalizations, and commitments to prior philosophical positions, frequently mar the work of even outstanding proponents of this conception of philosophy; and in the attempt to cleanse speculation of irrelevant and hampering accretions, dirty children are occasionally emptied with dirty water. The injunction to conduct analyses of concepts and theories with a sober eye on their contexts and functions is easier to preach than to follow, and it is apparently not difficult to believe that programs for such work are a sufficient earnest of achievement. Did space permit, chapter and verse could be cited to support these critical remarks. Nevertheless, the ideal which inspires this approach is not proved to be invalid or worthless because it is not embodied, completely and always, in the works of those who follow it; it is a difficult ideal, and one must be grateful for the fact that it is realized so often and so well.

Enough has perhaps been said concerning these four approaches to the philosophy of science to supply the reader with some basis for a comparative evaluation. But whatever be his judgment, it is clearly not necessary for him to reject any insights which writers cultivating different approaches have contributed to the sober understanding of the scientific enterprise. One should not forget Whitehead's warning that "a Nemesis waits upon those who deliberately avoid avenues of knowledge." The present essay, however, is obviously written with the conviction that a fruitful and illuminating type of philosophy of science will devote itself wholeheartedly to the contextual and functional analysis represented by the fourth of these approaches.

3

Philosophy and the American Temper

A NATION COMMITTED to a democratic way of life, but which prescribes for its citizens a set of beliefs concerning the nature of the cosmos and man's place in it, is a contradiction in terms. When permitted to reflect freely upon fundamental questions, men are stimulated to thought by diverse experiences and divergent traditions; and under these circumstances the world comes to be viewed from many different perspectives and portrayed in many different colors. But the freedom to develop alternative conceptions of nature and man, even though some of them may be grossly inadequate to their subject matter, is an essential part of the liberal, democratic tradition. There is accordingly no official American philosophy.

Nor is there, contrary to the tales repeated by many foreign observers, any unique American philosophy, stemming from native seed, nourished exclusively by native soil, and flowering in all but lonely profusion in all parts of the land. Only unfamiliarity with the variety of doctrines professed by academic teachers and reflec-

tive men of letters in America, or inability to resist an impulse for popular caricature, can produce such a claim. Ever since the settlement of the American continent, the commerce of ideas between it and Europe has been continuous, and the fundamental beliefs and habits of mind of American intellectuals have always been swayed by winds of doctrine coming from foreign shores. Even those modes of thought which have often been identified as distinctively American (for example, Puritanism, New England Transcendentalism, and Pragmatism) are historically related to intellectual movements which have flourished across the Atlantic—to British Deism and Empiricism, to the many varieties of German Philosophical Idealism, and to French Positivism and Voluntarism. There is at least a grain of truth in the witticism that philosophies which become moribund in Europe obtain a second lease upon life when transplanted to America. In any event, it is not possible to cite a type of philosophy current in America which has not its identical counterpart, or at least its unmistakable analogue, in other countries.

It would indeed be strange if it were otherwise. For although the political and social experience of America has in many respects been unique, contemporary America shares with western Europe a comparable literary and religious heritage, a similar social and economic structure, and above all an identical science. Geographical distance does not by itself produce differences in fundamental attitudes and beliefs. There is, to be sure, scarcely any evidence for the widespread view that systems of philosophy, like other manifestations of the human spirit, are simply the "reflections" of the prevailing economic structure of a society, and that there is therefore a point-to-point correspondence between the history of ideas and the socio-economic changes in the life of a people. But even in its most abstract reaches philosophy is surely a commentary upon experience, so that men steeped in similar cultural traditions and confronted with similar materials for reflections will in general adopt comparable modes of viewing their place in the world.

Nevertheless, when proper qualifications are made, it is not untrue to claim that a certain habit of mind and a set of general convictions characterize the writings of many professional philosophers in America which contrast sharply with the temper and the content of much foreign thought. But one must hasten to add that these convictions also contrast sharply with many intellectual currents

long and firmly established in the country, as well as with recent importations that promise to acquire a wide following: with the still influential survivals of the once dominant Calvinistic theologies; with the rosy-hued philosophies of progress, which for a time replaced these latter in popular favor and which continue to exercise a hold on many contemporary minds; with the current revivals of the Augustinian emphasis on the inherent sinfulness of man, and with the many calls to salvation through renunciation of wordly effort; with absolute idealism in its many forms, still not without defenders among the older generation of philosophers as well as among the younger ones; with a militant Thomism, which in recent years has acquired vigorous camp-followers in lay seats of learning; with phenomenology, stimulated to lush growth by the recent influx of European scholars; and with *Existenzphilosophie*, suddenly fashionable in literary circles because of French influence, though at the moment without energetic advocates among professional philosophers.

An adequate portrayal of the contemporary American scene would have to give more than incidental mention to these contrasting currents of thought. If they receive but scant notice in the present account, it is because the movement in philosophy to be briefly described appears to express more adequately than any of these the dominant temper of American life. For it is a movement which is profoundly influenced by the achievements of both theoretical and applied science, and which makes its largest appeal to a people filled with a sturdy faith in the power of intelligent thought and action to solve the problems besetting mankind. Without being narrowly practical-minded or philistine in its loyalties, without undervaluing the liberalizing functions of art, contemplative reflection and theoretical inquiry, it voices a frankly "this-worldly" philosophy. It expresses the aspirations of a people still young enough to believe that the good life can be achieved through an overt participation in worldly affairs, rather than through a melancholy resignation. It subscribes to the view that the things most dear to mankind are brought into existence and are sustained in their functions by natural forces; and it therefore finds only a romantic sentimentalism in the conception according to which men are wanderers in an inherently hostile world who must take their directions from supernatural guides. It counts among its adherents a number of the older gener-

ation of philosophers, but it also prides itself on the fact that it has the vigorous support of many of the keenest and best disciplined minds among the younger men. It is without question America's most significant contribution to philosophic intelligence.

This philosophy has been variously designated as objectvie relativism, functional realism, contextualism, naturalism, and process philosophy. Its central conceptions concerning man and nature have sometimes been identified with the pragmatism or instrumentalism of Peirce, James, Mead and Dewey. This identification is both mistaken and misleading. Although contextualistic naturalism has historical roots in pragmatism, and although Dewey himself is an exponent of both, those professing the former do not in general feel committed to the technical pragmatic doctrines concerning the nature of truth or the function of knowledge, and in the main they have no liking for the pragmatic or instrumentalist label. Indeed, the specific pragmatic conceptions propounded by James and Dewey, which caused such a flurry of excitement in the first quarter of the present century, now appear to be of little more than historical interest to the younger generation of American philosophers—in part, no doubt, because the intellectual storm raised by those conceptions has cleared the air of the traditional complacencies and barren self-deceptions against which pragmatism was a protest. Unlike contextualistic naturalism, it is problematic whether, except in some training schools for teachers and in centers of learning where the personal influence of the founders of pragmatism is still alive, pragmatism retains the expansive dynamism of a growing movement. Nonetheless, pragmatism is the matrix out of which, at least in America, contextualistic naturalism has emerged. For certain features of pragmatic philosophy that had been subordinated to its theory of truth and knowledge, have subsequently received an independent development; and fortified by fresh analyses of scientific method as well as by the findings of biological inquiry, they have been forged into a distinctive outlook upon man and nature.

Contextualistic naturalism is not a finished intellectual edifice, and perhaps it will never develop into one. Though much periodical literature indicates whence and whither it is tending, there certainly is no one treatise that could be cited as the *Summa* of its doctrines. However, many of the later writings of John Dewey, W. H. Sheldon's *America's Progressive Philosophy*, and various essays in

the recent collection entitled *Naturalism and the Human Spirit* (edited by Y. H. Krikorian) contain at least its basic ground plan. Those out of sympathy with it are likely to judge it as eclectic, loose-jointed, and at a number of essential points even distressingly vague. But those who subscribe to its main tenets believe that a working philosophy cannot be more tidy and architechtonically complete than the subjects of which it treats.

For a cardinal thesis of contextualistic naturalism is the essentially incomplete but fundamentally plural character of existence, in which no overarching pattern of development can be discerned, and in which qualitative discontinuities and loose conjunctions are as ultimate features as are firm connections and regular cycles of change. Events are recognized to have causes and consequences, without beginning and without preordained end. But though events are genuinely related to other events, they are not related to all events in the same way, not everything is relevant to the occurrence of everything else, and no events are related to others by logical necessity. Indeed, things may be caught up in some eddy of the flux of change which can alter their customary modes of behavior and transform them in a manner not always deducible from the known conditions of their normal existence. Novelty, contingency, and alternative possibilities of development, are accordingly basic features of nature. The disciplined human perspective upon the flux of events does not betray its observers, and the mixture of direction and chaos which that perspective reveals is not the superficial appearance of a fixed plan working itself out with inexorable logic.

In consequence, contextualistic naturalists exhibit a profound distrust of philosophic systems which attempt to catch once for all the variegated contents of the world in a web of dialectical necessity. They are keenly conscious of the limitations of purely formal analysis even when they engage in it. For they recognize that a logic, no matter how subtle, provides no warrant concerning matters of fact unless it is supported by controlled observation. Indeed, they sometimes show an almost pathological fear that those concerned with formal analysis may be deceived into supposing that nature is as coherently organized and as simple as are their intellectual constructions.

Nor does the familiar distinction between appearance and reality play any role in their thinking about nature, since the term "reality"

designates no inherently basic substance in the world but at best only a humanly valuable phase of existence. Accordingly, every quality and event is a genuine occurrence in some complex process or context, and possesses ascertainable relations and functions in that context. There is, however, no one context which is relevant to the occurrence of everything, there is no absolutely privileged context. It is this emphasis upon the contextual conditions for the occurrence and for the manifested properties of everything whatsoever—upon the fact that a quality is an objective constituent of nature even though its existence depends on the relations in which it stands to other things—which explains the adjective in the label "contextualistic naturalism."

It is inevitable that on these assumptions contextualistic naturalism should be vigorously anti-reductionist. Unlike earlier forms of naturalism, it maintains that the world contains at least as many qualitatively distinct features as are disclosed in human experience, and not a fewer number of them. The widespread view exemplified in such claims as that the playing of a Bach *Chaconne* is "nothing but" a scraping of horsehair across catgut, or that the human scene is "nothing but" an aggregation of certain allegedly "ultimate" elements, is therefore rejected as resting upon patent confusions. Modern physics and modern biopsychology are taken seriously by contextualistic naturalists, and current theories concerning the electrical structure of matter and the biophysical basis of human action are assumed to be well-grounded in competent evidence. But they fail to see any merit in the argument according to which, simply because complex organizations of the elementary particles of modern physics constitute the conditions for the occurrence of familiar events and processes, the characteristic behaviors of such complex wholes are indistinguishably identical with the behaviors of their differently organized parts.

It follows that the human scene is as much an integral part of nature, and as valid a subject for the philosopher's concern with basic features of existence, as is any of its other sectors. As contextualistic naturalism views them, man and his works are not inexplicable occurrences, incomparable in every respect with other natural processes. Men come into being and act the way they do only because of the operations of natural forces, and they perish or cease to behave as men when forces no less natural disrupt the normal or-

ganization of their bodies and of their fields of behavior. On the other hand, though the conditions of man's life are thus continuous with the rest of nature, his behavior exhibits features which are discontinuous with other parts of existence. For man is not simply another odd item in the inexhaustible catalogue of created things. He possesses the apparently unique gift of an inquiring mind, which enables him not only to act under the compulsion of internal springs of action and external pressures, but also to direct his impulses and to master many of the forces in his environment.

This gift of intelligence man owes to the organization of his body and the character of his environment, and no supernatural agency or disembodied soul is required to explain it. It is this organization, complicated by the structure of his surroundings, which also accounts for men's moral and evaluative behaviors. The possession of needs and preferences, and the exercise of reflection upon them in the interest of fulfilling and harmonizing them, are as natural to man as is, for example, the property of a magnet to repel or attract another magnet. In any event, it is in the radical plurality of men's needs and in the limitations which their physical and social environment impose upon their fulfillment, that contextualistic naturalism locates the source and urgency of moral problems. Accordingly, it does not conceive the primary moral problem to be that of discovering or actually instituting some fixed set of ethical norms valid everywhere and for all time. For basic moral problems are plural in number and specific in character, and are concerned with the adjustment, in the light of causes and consequences, of competing impulses occurring in specific environmental contexts. There can therefore be no general or final solution to the moral predicaments of mankind; the moral problem is the perennial one of finding ways and means for eliminating needless suffering and for organizing in a reasonable manner the energies of men.

The advocacy of a responsible intellectual method, especially in matters pertinent to social and ethical issues as well as in philosophy, is thus an emphatic strain in the writings of contextualistic naturalists. For reliable knowledge is the end-product of a reflective process, involving the use of experimental controls over ideas which initially have the status of tentative hypotheses; and it is this procedure which must be employed if reliable knowledge and reasonable evaluations are to be attained in the settlement of social and

moral conflicts. This method of science supplies no guarantees against error, it does not preclude alternative solutions to problems, and it certifies none of its conclusions as eternally valid. But since it is in essence a self-corrective method, and involves the continued criticism of its findings in the light of evidence capable of public inspection, it is a method which can discover its own errors. It is in any case the sole method which has historically shown itself able to yield intellectual and practical mastery over various segments of nature. From the systematic extension of the use of this method to the problems of men, contextualistic naturalists confidently anticipate an increased moral enlightenment.

Compared with many fashionable philosophies contextualistic naturalism is almost prosaically sober. It contributes to the current intellectual scene no apocalyptic visions, no thunderous absolutes, no unshakeable certainties. It offers no spectacular promises of salvation. It is essentially scientific and secular in temper, but confident that the concentration of scientific methods upon specific problems will yield a rich harvest of genuine knowledge. It is sane and reasonable at a time when the tides of irrationalism run high in the world and when substitutes for the Appolonian virtues are at a premium. It expresses the convictions of a people confident that a bold but disciplined intelligence is still a creative power in the world.

4

Charles Peirce's Guesses at the Riddle

WITH THE PUBLICATION of his *Collected Papers*[1] by Harvard University a measure of justice is being done to Charles Peirce, long neglected except by a select few students. Unfortunately, the handsome volumes of this edition can not undo the damage which both Peirce and philosophy have suffered when he was not permitted, except for a brief period, to receive the intellectual and literary discipline which regular university teaching would have given him. His was an erratic temperament, as his writings amply testify. His conception of philosophy was architectonic, and he tried to write it in a grand manner; but most of the hitherto unpublished material which has appeared thus far is obscure, uneven in quality, and, in spite of the best efforts of his editors, unsystematic. As he

1. *Collected Papers of Charles Saunders Peirce.* Edited by Charles Hartshorne and Paul Weiss. Cambridge: Harvard University Press. Vol. I, 1931. Pp. xvi + 393. Vol. II, 1932. Pp. xii + 535.

himself remarked in 1903, "All that you can find in print of my work on logic are simply scattered outcroppings here and there of a rich vein which remains unpublished. Most of it I suppose has been written down; but no human being could ever put together the fragments. I could not myself do so." It is not surprising, therefore, that the writings in these first two volumes which are best from the point of view of clarity, consistency, and organization, are the articles published during his own lifetime.

Until all the ten volumes will have been published it is impossible to evaluate adequately even the first two. Their contents will suggest, to anyone familiar with the literature of the last quarter-century, many of the central ideas of better-known writers. It is easy to find something of Husserl, Whitehead, Santayana, Wittgenstein and the logical positivists, Hilbert, Brouwer, and others, in the hints thrown out by Peirce, but left undeveloped. But whether these "anticipations" can legitimately be read *out* of Peirce, instead of read *into* him, only the later volumes will perhaps definitely decide. It is not impossible that he may become everything to all men, and that rationalists and empiricists, naturalists and idealists, realists and nominalists, may all read him for support as well as stimulation. Peirce was often his own audience,[2] and he could afford to be voluminous, repetitious, sketchy, and obscurely subtle. But to others it is not always evident what value much of his analysis has for clarifying problems of philosophy or science. His readers get an impression of someone packing and preparing for an important tour, but who somehow never crosses the gates of his own familiar city: we are shown the itinerary and given glowing promises of what is in store for us, but alas! too frequently the manuscript breaks off just before the train is scheduled to leave.

Not even a complete summary can be given, in brief compass, of the contents of these volumes. This review must confine itself to a statement of just three important themes, and the interested reader must be left to examine at first hand many of the interesting variations which Peirce embroiders upon them.

2. He supplied one manuscript with the following heading: "Notes for a Book, to be entitled 'A Guess at the Riddle,' with a Vignette of the Sphynx below the Title. And this book, if ever written, as it soon will be if I am in a situation to do it, will be one of the births of time."

1.

The Peirce who is familiar to readers of his published writings is usually the mathematical logician and student of scientific method. But his scientific interests seem to have been subordinate to a far-flung metaphysics, and the substantial part of Volume I is devoted to an analysis and a speculative use of the categories. He desired to draw up a list of fundamental categories such that for a long future the intellectual achievements of man would appear simply as corroborative details, and one of his earliest published papers was already concerned with this task. Unfortunately, Peirce had a fondness for trichotomous divisions, and so he recognized just three fundamental categories: Firstness, Secondness, and Thirdness. But Secondness is analyzable into two species, the second of which is again divisible into two species, and so on; while Thirdness is analyzable into three kinds, the second of which is divisible into two varieties, and so on, and the third of which is divisible into three varieties, etc. His thought appears in a very artificial form indeed, although he had enough sense of humor to see how crack-brained his triadic scheme must appear to the general reader (1.364).[3]

Peirce has not definitely stated what status he assigns to the categories. But they are frequently proposed as the directly apprehended logical simples into which the complexity of whatever is presented may be analyzed. For his object is to establish a science of phaneroscopy (phenomenology) which would describe the total of whatever is present to the mind in any way (phaneron), irrespective of the "realities" to which the phanerons correspond or of their physical and psychological conditions (1.287).[4] Now the study of the phaneron, Peirce claims, shows it to contain three indecomposable elements or aspects, which are the three categories. But he was careful to point out that these elements are not *existentially separate*, and that their analysis consists only in tracing out the relations between features isolated logically (1.294). The elements of

3. The references are to volume and paragraph.

4. This approach has many obvious affiliations with the "presuppositionless" studies of essences by Descartes, Husserl, and Santayana, although in the light of Peirce's devastating critique of the Augustinian-Cartesian philosophy in his earliest papers, too much can not be made of this similarity. And after the nature of Thirdness has been discussed it will be seen how full of presuppositions Peirce's analysis really is.

the phaneron are indecomposable, because they show no differences in internal logical structure, although there are differences in the structure of their possible compounds (1.288, 1.291). The categories seem to be, therefore, factors continually present in all experience (1.134), and indissolubly intertwined with one another. We may *distinguish* the factors from one another, but we can not, even in imagination, *dissociate* them from one another or from other conceptions. We may indeed *prescind* or *suppose* some from the others in the same way as we prescind or suppose space from color (1.353); but we must never regard them as atoms out of which anything is constructed, since they are always only distinctions in everything presented (1.286).

Although the categories primarily characterize the phaneron, Peirce also regards them as representing congenital tendencies of the mind, and admits that so far he agrees with Kant. But he makes a careful study of the facts in various fields in order to confirm the pervasiveness of three modes of being (1.374); for he regards the list of categories as "a table of conceptions drawn from the logical analysis of thought and regarded as applicable to being" (1.300). It is difficult to evaluate his performance, since the analogies he finds between features in different subject-matters are very tenuous, the argument is often *post hoc*, and he himself points out in one case at least that the categories he finds in one domain do not correspond precisely with those in another (1.452). He uses (and thereby abuses) propositions in the general theory of conics to illustrate fundamental metaphysical distinctions, and the outcome is very mystifying metaphysics indeed (1.362). Finally, it must be remembered that Peirce admits his triad of categories does not exhaust all the categories (1.525), so that there is reason to suppose that his predilection for trichotomies was nurtured by his studies in the logic of relations; although what the intimate connection is between these studies and his metaphysics Peirce leaves the reader to guess at random.

Let us turn to the categories themselves. The Category of Firstness indicates the aspect of freshness, life, freedom, variety, and spontaneity in the phaneron. Thus it would seem that Firstness presents the phase of atomicity, specificity, or sheer quality in experience. A quality is just what it is, and in its sheer immediacy is utterly disconnected with any other quality. On the other hand

Peirce regards Firstness as a "pure nature" or quality to which embodiment and localization are essentially foreign (1.303). A quality is eternal, independent of time or of any realization (1.420); it is simply a "may-be," whose nature is that it *might* be a specific moment in a phaneron. Nevertheless Firstness is not a general or universal, since to be a universal involves reference to embodiment and localization (1.304).

It is not easy to reconcile these various characterizations of Firstness. Peirce seems to shuttle back and forth between regarding Firstness as the absolutely specific, immediate quality in the phaneron, as a determination which is not an "abstract suchness" (1.303, 1.306) on the one hand; and on the other hand, regarding Firstness as pure potentials, involving a variety and spontaneity which "is not definitely there," mere "may-bes" not necessarily realized (1.373). He thus alternates between conceiving Firstness as being simply a distinction within the phaneron, and conceiving it as a disembodied but substantial essence. Firstness is at the same time a mere "possibility" and the element of spontaneity; it is declared to lack generality, and also to be of the nature of a general, capable of further determinations (1.447). Peirce himself acknowledged that "possibility" was a misnomer for firstness, since it mistakenly suggested a relation to what exists (1.531). But he adds a final touch to these confusions when he identifies Firstness or quality, not with immediate, physically inefficacious quality, nor with an abstract isolated possibility, but with the *power* bodies have of determining future manifestations of inefficacious qualities (1.422). Firstness is then conceived as a dynamic relation, and so an element of law or regularity (Thirdness) is introduced into what started out to be the element of spontaneity.

Further difficulties arise when the connection of qualities with feelings is examined. A feeling for Peirce is an element of immediate consciousness, a quality of it which is not an event (1.306); and a quality in general is something which is vivid and conscious (1.357). There is thus a tendency to identify qualities with feelings, and so it is only a short step to impute feelings to whatever has qualities, for example, to inorganic matter. Indeed, "dead matter" can excite feelings in us, only because dead matter isn't dead at all (1.311). Consequently, Peirce must be included among those who use with abandon an uncritically formulated principle of continuity.

Like can come only from the like, and so the qualities which humans enjoy are communicable and find their counterparts in non-human situations. "I am confident," Peirce declares, "that a bull and I feel much alike at the sight of a red rag" (1.314). Far be it from me to question this assertion, especially since what it means is most obscure. I do not see, however, how it and similar statements are reconcilable with Peirce's logical pragmatism. He himself declared that in immediate experience there is no *knowledge*, so that if Firstness indicates the element of spontaneity, every description must be false to it (1.357). And I think this is true, because of that which is wholly immediate nothing whatsoever can be said, for the simple reason that the wholly immediate is not the subject-matter for any saying.

We turn to the second category. Secondness indicates the element of brute fact, struggle, actuality. It points to fact, within which distinct elements stand in opposition to one another, for all existence is always a matter of blind force. Facts are determinate with respect to every character. They involve physical things acting upon one another *hic et nunc;* these physical things *are* the sum of all the consequences of their so acting here and now. But Peirce points out that the individual mode of behavior indicated by Secondness is not *determined* by any law (1.440); a stone falling here and now is not *made* to do so by any law of nature (1.323). Hence the proposition that every event is precisely determined must be abandoned, since it is only approximately true (1.402).

Secondness thus points to the element of contingency, of something "accidently actual" in the phaneron (1.427). Peirce would have been sympathetic with recent attempts to revive causality as a distinct category not analyzable into uniform succession (1.325). For Secondness denotes a necessity which is natural, and also unconditional, forceful, ultimate, a quality of operative events. At the same time, Secondness points to a radical indeterminism in the behavior of nature, so that Peirce anticipated in general outline the statistical interpretation of natural laws.

Unfortunately, but perhaps inevitably, Peirce's discussion of Secondness is almost entirely in anthropomorphic language. We may cite as examples his characterization of fact as something which "fights its way into existence; for it exists by virtue of the oppositions which it involves" (1.432); and "the existence of facts consists

in fight" (1.435). This anthropomorphism Peirce often regards as a virtue, and once again a principle of continuity is employed to read strife, struggle, effort, which are features of human life, as generic traits of all nature. But where shall one draw the line—why not read desire, revulsion, love, into every nook and cranny of the world as well? Peirce, like many contemporary thinkers, did not hesitate at times to draw just such conclusions. Now the world may, indeed, be populated with many more qualities than we ordinarily suppose. But why should we uncritically admit them, at the behest of a dubiously stated principle of continuity? Why should we overlook the fact that the qualities we experience are functions, in part at least, of the interaction of our own bodies with the rest of nature, and that they can not therefore be imputed to segments of the world where such interaction is not going on?

An equally serious shortcoming is Peirce's failure to analyze the meaning of contingency, and to state how he conceives an event to exhibit a contingent aspect and also to illustrate a uniformity. Is the contingency of an event an absolute character of it, an immediate quality of a *hic et nunc* operation; is it a relational character, so that an event has a contingent aspect with respect to one set of circumstances but not to another set; is it synonymous with the sheer plurality and variety of things; or is it all these things and something else besides? This problem is especially important for Peirce, because as we shall see, uniformity is taken by him as a temporally derivative character of events.

Let us examine the final category. The category of Thirdness introduces the element of determination by law. A law is the mode of being which consists in *future* facts (of Secondness) taking on determinate general characters (1.26); a law is how an endless future must continue to be (1.536). Thus, Thirdness involves the idea of something general, which can not ever be completely actualized and so refers to an indefinite future. Thirdness or law, according to Peirce, is akin to thought; it can be produced and grow, it can be communicated and is general. It addresses itself to two realms: as general, it concerns itself with the realm of possibility or quality; as factual, it refers to the actual world (1.420). Since Thirdness is involved in all communication, the correct analysis of the category leads inevitably to the study of the nature of

signs. But we will examine this aspect of the matter in the next section.

At this point, let us see the use Peirce makes of his categories in his cosmogonical speculations. According to him, every sound metaphysics must be evolutionary. This dictum is based largely on the further dictum that what needs to be explained is not the variety, freshness, and unpredictability of things, but the uniformities which they exhibit. Uniformity must be "explained" by showing how, out of a primal spontaneity, together with a *tendency* in the flux to take on habits, laws or habits have *evolved* (1.175). In a sense, therefore, Thirdness is not as "ultimate" a category as Firstness, since regularity grows out of pure chance or Firstness (1.407). In the infinitely distant past there was no law, only chaos together with a tendency toward uniformity or the elimination of indeterminacy (1.409). Hence the three active elements in the universe are chance, law, and the tendency for things to take on habits

Is it possible to make a coherent scheme out of all this? In the first place, let us recall that the categories should be regarded as *distinctions* within the phaneron, not as active elements out of which whatever exists is to be constructed. However, if we also recall that phaneroscopy has nothing to do with the question of how far the phaneron corresponds to any reality, what meaning shall we assign to Secondness and Thirdness which specifically involve reference to the actual world? Can anyone, least of all Peirce, maintain that the nature of law is to be learned independently of the operations of "real" beings and of the use of the discursive methods of the sciences? Indeed, if there is nothing in immediate consciousness except feeling or quality (1.317), how can the phaneron exhibit elements of Secondness and Thirdness? Peirce himself declares that law unrealized is merely a pure first, a quality possessed, and is not a "genuine" law.

Secondly, is the idea of law as something active anything other than Plotinian and Schellingian poetry? It is true that Peirce calls Thirdness a law only when we contemplate it from the outside, while if "we see both sides" it shows itself to be akin to thought (1.420). But change of terminology does not always solve a difficulty, and Peirce does not show how the radical indeterminacy of fact is compatible with the *activity* of thought or law in determining what future facts are to be.

Thirdly, what is to be said for the dictum that uniformities, and only uniformities, require explanation? Is it not patent that we sometimes seek to explain a regularity and at other times the lack of regularity? For what, anyway, is an explanation? Is it not the case that we "explain" an event or law when we exhibit it as an element in a more comprehensive order or uniformity? Thus we explain why a glass breaks when hot water is poured into it, by showing that *types* of relations, like the flow of heat, the elasticity of a substance, and the unequal expansion of a substance, are exhibited in the explicandum. These types, if they are to explain, must be pervasive modes of behavior, must be what are called "regularities." Peirce himself is compelled to take note of them. He can not possibly get order and connection by simply taking them as *growths* from a primal chaos; he must take for granted some form of order, e.g., a tendency in things to take on and conserve habits. For the hypothetical chaos is completely unintelligible, and can not be the explanation of anything. One may explain a specific law by showing it to be the consequence of a more inclusive order. But one can not explain order as such, since it is the principle of explanation of everything else. What order we take as primary in any inquiry depends upon the state of our knowledge and the demands of the problem; *some* order we *must* assume, just as *some* plurality *must* be taken for granted if anything is to be explained. Peirce was motivated in his cosmogony by a bias toward a spiritualistic interpretation of the cosmos, and by an understandable reaction to philosophies of his day which attempted to derive the variety of things from a primal homogeneity. The moral is that it is just as much a mistake to deny an ultimate specificity and variety, as it is to deny an inexpugnable order. It is instructive to compare Peirce with Spinoza. Both recognized nature to possess a structure and also to contain an inexhaustible variety. Peirce chose to emphasize the latter and to regard the former as derivative. But I think Spinoza was both sounder and wiser when he included in Part I of the *Ethics* Prop. 15 as well as Prop. 28.[5]

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5. Prop. XV. "Whatever is, is in God, and without God nothing can be, or be conceived."

Prop. XXVIII. "Every individual thing, or everything which is finite and has a conditioned existence, cannot exist or be conditioned to act, unless it

Finally, one must seriously question Peirce's facile reduction of laws of nature to "habits." No doubt there is an analogy, often a fruitful one, and no suggestive hypothesis should be neglected. But do we know the nature of human habits so well that we can pretend to throw light on the nature of laws by using the former to explain the latter? What real evidence is there for Peirce's assumption that natural laws are like habits which human beings take on and occasionally drop? It is true that almost all our scientific terms, even the most neutral ones, are infected with meanings carried over from the specifically human scene. But this should not be taken as a license to exercise one's speculative fancy and so regard processes in all parts of nature as simply human processes writ large. The issue is not between affirming immutable laws of nature and affirming the contrary. The flux is what it is, and why should a naturalist on *a priori* grounds deny anything of it? But whatever the flux is, the issue is to examine any allegation concerning it with scrupulous care, in philosophy as well as in science. And if it is alleged that laws grow and change, while at the same time laws are not identified with any segment of the flux, especial care should be taken to determine the sense in which laws "change," in view of the fact that change is taken as a fundamental character of the flux itself.

2.

Although the triad of categories is a persistant melody running through most of Peirce's writings, many of his doctrines can be understood better in connection with his theory of signs. The magnificent series of papers in the *Journal of Speculative Philosophy*, in which he broke with the Cartesian tradition dominating modern philosophy, was inspired by his recognition of the function of signs in thought. Like Aristotle, Peirce was impressed by the fact that language is the medium in which the rationality of the world becomes expressed and communicated; and he went so far as to say that "the woof and warp of all thought and all research is

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be conditioned for existence and action by a cause other than itself, which is also finite, and has a conditioned existence; and likewise this cause cannot in its turn exist, or be conditioned to act, unless it be conditioned for existence and action by another cause, which also is finite, and has a conditioned existence, and so on to infinity." Spinoza's *Ethics*, Part I, Elwes translation.

symbols, and the life of thought and science is the life inherent in symbols" (2.220). "My language," he declared, "is the sum total of myself, for the man is the thought. The identity of man consists in the consistency of what he does and thinks, and consistency is the intellectual character of a thing, i.e., is its *expressing* something." Representation or the operation of signs (mediation or Thirdness) is therefore one of the fundamental categories, and to its study Peirce devoted his best energies.

Peirce's ideas on signs are the most difficult and obscure of all he set down on paper. Only a meager outline is here attempted. Anything whatsoever (e.g., physical object, image, quality, thought) may be a sign, so that signs differ from one another in several important ways. But in any case, every sign involves a triadic relation. For in every case where a sign functions, there is first, the sign with its own material quality; secondly, there is the object *of* which it is a sign, and which it represents in some respect (the *ground*); and thirdly, there is the thought *to* which it is addressed, the interpretant *to* which it is a sign. In Volume II Peirce discusses each one of these three aspects of the sign, each aspect yielding three divisions.[6]

And first, when we attend to the material quality of signs, we find three types: a sign may be a pure quality or possibility, something which as a pure quality is disembodied, even though it can not actually function as a sign until embodied (a *qualisign*); or it may be an actual existent, a physical thing or event (a *sinsign*); or finally, it may be a general law, rule, or habit (a *legisign*). Every sinsign clearly must involve qualisigns, while every legisign functions through sinsigns which are instances or "replicas" of it. Thus, in one sense, each time "three" occurs on a page there is a distinct word, each being a sinsign; but in another sense, these distinct sinsigns stand for the very same word (legisign), which is a general rule or convention for the production of sinsigns. All conventional marks are signs not because they can be seen or touched, but because they are replicas of a type which signifies *through* its instances (2.244).

Secondly, when the sign is studied with respect to the relation between it and its object, the following three types are found: a

6. In a later volume he will distinguish ten trichotomous divisions, giving sixty-six independent classes of signs.

sign is an *icon* when it refers to an object because of its own intrinsic features, which nevertheless it possesses in common with its object (e.g., a diagram or picture); it is an *index* when it is really (dynamically) affected by its object (e.g., a street cry, the rolling gait of a man as a sign of his being a sailor, or a weather-cock); it is a *symbol,* when it operates to be interpreted by virtue of a law or association of general ideas (e.g., a proposition, all conventional signs). Peirce regarded this as the most important division of signs. The only way we have of communicating and thinking, according to him, is by means of direct or indirect use of icons (2.278). But icons generally function as constituents of indices, which in turn are constituents of symbols, and symbols, being general, involve laws referring to the indefinite future, and so require legisigns and sinsigns to function (2.293).

Thirdly, when the sign is examined with respect to what its interpretant understands it to represent, the following three types are found: when the sign is taken as representing a kind of *possible* object or quality, when it represents an object merely with respect to its characters, it is a *rheme* (in modern terminology, a propositional function); when it is taken as a sign of actual existence, when it represents its objects with respect to the actual existence of the object, it is a *dicisign;* when it is taken as a sign of law or regularity, it is an *argument* (2.252).

If we combine these three sets of distinctions in all possible ways, we get twenty-seven (i.e., 3^3) classes of signs, only ten of which are independent. Peirce discusses them with great subtlety. We can not report his analyses, but turn instead to some of the applications he makes of his studies on the rôle of signs in discursive reasoning.

(a) *The nature of meaning.* It is not intended to add here another specimen to the sixteen meanings of "meaning" captured by Ogden and Richards. I only wish to point out that most analyses of meaning have gone off the track because they identify meaning with some essense, image, or thought, instead of studying the structure of the situations in which meanings occur. Peirce's discussion of signs should make us aware that "meaning" is a triadic relation involving a sign, its object, and its interpreter, so that an exclusive concentration upon one of these terms must of necessity yield an inadequate analysis of the nature of meaning. It may be thought that the specific introduction of the interpreter as a factor in the

analysis of meaning, involves the introduction of irrelevant psychology which will only becloud a very beclouded problem. But quite the contrary is the case. For this analysis isolates the different factors in a meaning situation, allows us to explore the general conditions under which signs may adequately represent their objects irrespective of any specific interpreter, and then emphasizes the ineradicable rôle of the interpreter. Thus we can discover that a sign must be a "logical map" or icon of its object if it is to represent it adequately; and we can also consider the social factors which enter into the activities of the interpreter which determine how he interprets signs, without however altering the validity of the first analysis. Meanings are not qualities or essences, they are the structure of certain situations in which such triadic relations are exhibited.

Consider, furthermore, the problem as to the nature of propositions. A proposition is sometimes identified with judgment, understood as a psychological act, sometimes with a fact or state of affairs; sometimes with a substantial, non-temporal, non-spatial entity or essense; sometimes with a sentence in all its particularity. Peirce, on the other hand, is able to avoid both the hypostasis and the psychologizing of propositions by analyzing them as *dicent symbols which are legisigns.* Hence a proposition is a sign, and requires a three-fold reference: it is a sign which is connected with its object by an association of general ideas; it is dynamically connected with its object if it is true, and is *interpreted* as representing a situation *located* in the realm investigated; and it also has a determinate material quality, determined by a general rule or convention, so that "*cogito*" and "I think" stand for the same proposition.

It is also relevant to mention in this connection that most of the recent work in symbolic logic has devoted exclusive attention to the *iconic* feature of signs, and has neglected their very important *indexical*, *symbolical*, and *argumentative* aspects. The dissatisfaction which many philosophers have felt with symbolic logic may perhaps be explained on this ground. And it is not too much to say that an adequate philosophy of logic will not be achieved until the neglected features of signs receive a complete study.

(b). *The doctrine of fallibilism.* Every sign addresses itself to some thought or interpreter, and this in turn, according to Peirce, is a sign, which must address itself to another thought, and so on

without limit. This indicates the nature of all discursive thought, and it is largely for this reason that he denied knowledge to be immediate apprehension. All cognition requires mediation, interpretation, and reference to an indefinite future. There can be no absolute finality or complete certitude, since the process of interpretation is an unlimited one. The method of science is self-corrective. We can not doubt all things at once; but without being dogmatic or sceptical, we can construct hypotheses, submit them to empirical tests, reconsider the hypotheses and tests in terms of other hypotheses and observations, and so on endlessly. Thus while the sciences aim at exactitude, universality, and complete certitude, these are ideal limits which can never reach, and which it is suicidal for them to claim to have reached. All our knowledge, in the natural sciences as well as in mathematics, is fallible.

The import of this doctrine will be clearer if we bear in mind that the complete certitude Peirce denies we can attain, is *not* a psychological state of strong conviction, but is a complete *logical* ground for the propositions we assert. The following considerations will bear this out.

(1) Peirce makes clear that it is the fallibility of *discursive thought* which he is asserting, and that he is not denying the immediacy of direct experience. Kant's sharp distinction between observation and reflection he believes is mistaken, because all significant observation involves interpretation, so that reasoning does *not* begin *after* observation is over (1.35). The consequences of this criticism, as everyone knows, are momentous for the theory of science. And he has a crushing retort to those who claim absolute certainty for the objects of direct experience: "Direct experience is neither certain nor uncertain, because it affirms nothing–it just *is*. . . . It involves no error, because it testifies to nothing but its own appearance. For the same reason, it affords no certainty" (1.145).

(2) The claim to infallibility is only a hindrance to inquiry, since what we want to know concerning any proposition is whether it is well supported by evidence. The claim to finality, however, confuses the question of the logical value of the evidence with the question as to the psychological certitude we may possess. Accordingly, since the first rule of reason is "Do not block the way of inquiry" (1.135), we may dismiss as obstacles the claims that we have absolute certitude, that there is something we can never know, that some-

thing or other is basic or absolutely simple, and that some law or other has received its perfect formulation.

(3) It is a very crude analysis which declares that things are sharply cut off from one another, like the traditional pictures of atoms as billiard balls. The contrary is the case, according to Peirce, for "all things swim in continua." Our concepts, too, shade off into penumbral regions of indeterminacy, so that our knowledge "swims in a continuum of uncertainty and indeterminacy" (1.171). Hence the "laws of nature" must be interpreted as describing the approximate behavior of large groups, and do not describe without serious qualifications the behavior of their individual members (1 156).

(4) A pragmatic theory of meaning involves an implicit reference to the future in every judgment about matters of fact. To call a piece of stone *hard*, is to *predict* how it will behave and what effects it will have in the future (1.615). Hence every present judgment is subject to correction, since it involves an endless series of verifications. Science is therefore not so much of a body of established truth, as a diligent *search* or *process of inquiry* into truth (1.44).

Against the doctrine of fallibilism many thinkers will no doubt raise the objection that we can not be said to "know," unless what we know is in fact the case. Anyone claiming to "know" that the earth is flat, it will be said, claims what is false, for we could *know* that proposition to be true only if the earth were indeed flat. Hence, if there is such a thing as knowledge, it must be absolutely certain; on Peirce's view, however, it will be urged, we may have opinion and belief, but never authentic knowledge, since knowledge is not subject to revision. To this objection there is only one reply. Knowledge so defined that when we have it we can not possibly be in error, is the ideal goal to which we strive; but it is Peirce's claim that we have not yet anything to illustrate such a knowledge beyond every possible doubt. It may well be that certain propositions claimed as knowledge will never be significantly challenged. The indubility that attaches to such propositions comes, however, not from any analysis of the nature of knowledge nor from a psychological sense of certitude, but from the fact that they are supported by logically overwhelming evidence. Nevertheless, in all discursive thinking there is always the possibility that we are mistaken in what we allege to know. There are therefore no ultimate first principles intuitively

reached which are the bed-rock of all certitude. There is only science as a self-corrective process, steering midway between a stultifying scepticism and an unimaginative dogmatism.

(c) *Logic and mathematics.* But what does Peirce do with logic and mathematics, it will be asked. Can he successfully extend the principle of fallibilism to apply to them? The attempt to do so is indeed one of the most interesting features of Peirce's writings on logic; and it merits careful study because he was bitterly hostile to the psychologizing logicians of his day.

Logic in its broadest sense is understood by him to be general semeiotic, or the science of what must be and ought to be the nature of true representations or signs (1.444, 1.539). Logic is thus inextricably related to language. Corresponding to the three-fold nature of signs, it has three departments: *critical logic*, the theory of the conditions for the *truth* of signs; *speculative grammar*, the theory of the nature of signs in all their aspects; and *speculative rhetoric*, or the theory of the conditions of reference of signs to their interpretants (2.93). The content of speculative grammar has already been indicated; and apparently Peirce wrote nothing systematically on speculative rhetoric, in spite of his belief that general methods could be stated for attacking the solution of problems (2.108). There remains to indicate something concerning the nature of critical logic.

The task of critical logic is the classification of arguments with respect to the form of their constituent propositions and the frequency with which true premises of a determinate type yield true conclusions. Each argument requires a *habit* of reference, which when formulated is the leading principle or canon of that *type* of argument. These habits are *general* since they refer to an indefinite future, and in a determinate class of arguments commit us to few or no false conclusions (2.148, 2.446). Some leading principles are perfectly general or formal: they are involved in every set of premisses, and are the logical principles *par excellence;* others have a more restricted generality, and are material principles (2.466, 2.589).

The consequences of taking leading principles or generalized habits as the basis for valid inference are many. It enables Peirce to supply an objective, matter-of-fact foundation for all inference, and so indicate the relevance of material considerations in formal logic. The naturalistic strain in him shows itself at its best in his discus-

sions of the metaphysical grounds of logical canons of inference (2.710 ff). It also enables him to treat in a unified manner both necessary and probable reasoning, since the former is a limiting case of the latter when the leading principles employed have relative truth-frequencies whose values are always unity (2.696). It permits him to do justice to the purely mechanical element in inference, and admit that logical machines perform inferences even though they do not think (2 59). And it permits him to avoid a crude empiricism with respect to logic, by associating the canons of deductive or explicative inference with the analysis of *signs*, so that he can retain his naturalism and still admit that the experiential element in logic is all but nil (2.65).

Nevertheless, not everything that Peirce says about inference is crystal clear or consistent. There remains an unbridgeable gap between probable and necessary reasoning which he reluctantly admits when he asserts that a class of arguments whose relative truth-frequency is unity, does not *therefore* belong to the class of necessary inferences (2.369). And when he asserts that the process of substitution, so vital in formal reasoning, is non-inferential, simply because the propositional forms employed have no *specific* meaning or reference, he exhibits serious limitations in his understanding of the distinctive marks of inference (2.496).

Let us turn, however, to his analysis of deductive reasoning and to the incidence of the doctrine of fallibilism upon it. We can best do this in connection with his discussion of mathematical reasoning. Peirce was profoundly influenced by F. A. Lange's *Logische Studien* and always maintained that both logic and mathematics were "observational sciences." This *aperçu* depends on recognizing that all reasoning is by means of and upon signs. In mathematics, for example, we construct a diagram (i.e., any complex sign) according to a general rule; we observe relations between parts of the diagram not stated explicitly by the rule; we show that the diagram is a fair sample of all the diagrams determined by the rule, so that these relations hold always (or at least in a certain proportion of cases) for any such diagram; and finally we state these relations in general terms as the necessary (or probable) conclusion of the investigation[7]

7. In this emphasis upon the rôle of diagrams Peirce anticipated much of the mathematical philosophy of Hilbert, although his motivation is clearly different. Like Hilbert, he denies that mathematics ever requires the aid of

(1.54, 1.66). In the sense, therefore, that explicative reasoning is diagrammatic, it requires observation, since we must construct diagrams, at least in imagination, and *observe* them (1.240); and it will be seen that the discussion does not depend on the status of the controversy between opponents and defenders of "imageless thinking." Hence, mathematics is no more infallible than other positive sciences of observation, and every mathematical inference is accordingly a matter of probability (1.248, 2.192) even though it may be beyond every reasonable doubt.

Whatever else may be said of this extension of the principle of fallibilism to mathematics, Peirce has rendered a service in pointing out how logically insecure are all claims for the self-evidence of mathematical propositions. The history of logic and mathematics amply bears out the substance of Peirce's warnings against an arrogant infallibilism; it shows that here as elsewhere, general principles are but slowly apprehended, require to be carefully tested out, and the limits of their application are never completely free from doubt. Nevertheless, there is a good deal of confusion in his contention that mathematical reasoning is "probable." His illustrations indicate that he means by that characterization simply that we, and thousands of others, may have blundered in adding a column of figures or in deducing the consequences of a set of premisses. But there is an important difference between the assertion that "$7 + 5 = 12$" is only probable because *we* may have erred, and the assertion that "all men are mortal" is probable because the evidence from biochemistry and physiology is incomplete and because there is a reference to a contingent future. In the first case we are dealing with the connection of relations having the utmost generality possible and not subject to change; in the second, the subject-matter is a segment of an incompletely determined flux. And Peirce himself, as we shall see, stood out firmly against confusing probability with ignorance.

Peirce's views as to the nature of mathematics are amazingly up to date. He saw clearly that pure mathematics is a hypothetico-deductive science, that it does not make categorical assertions about what exists (1.53), and that all *a priori* arguments about matters of

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logic (1.247, 2.191), although this dictum seems to depend upon his taking "logic" in this context to be a science of *fact*, and not the formal science of the consequences of hypotheses.

fact are just rubbish (2.137). But he did not always see the implications of these insights. Thus he thinks that "two straight lines can have only one intersection" is an evident proposition (1.130), and that the postulates of geometry are only approximately true (1.131). He can not therefore be regarded as having anticipated Poincaré and Duhem in their profound studies on the connection between applied mathematics and physics.

(d) *Nominalism.* It is clear from what precedes that Peirce could not agree with those for whom the only mode of being is that of individual objects which crowd out a place for themselves by sheer force. Such a view he characterized as nominalism, and he called himself a realist because he recognized the mode of being of law or Thirdness.

Peirce takes great pains to make clear that by a law or universal he did not mean a "thing" or a "particular." When a man believes that the property *hardness* is not invented by men as the word "hard" is, he says, "but is really and truly in the hard things and is one in them all, as a description of habit, disposition, or behavior, *then* he *is* a realist" (1.27n). The mode of being of a law consists in the fact that future acts will take on a determinate general character. Whatever is general, is something which is the *object*, not the creature of thought. And it can not be identified with any finite set of particulars, since "its being is in the instances which it will determine" (2.249).

Peirce's realism is thus of the "moderate" kind. Universals or laws are not capable of localization in space and time, nor are they something added to the particulars so localized. They are the modes of behavior, and the relations between modes of behavior, of things in space and time. In spite of his flirting with the idea of "pure possibilities," in spite of his saying that a typical mathematician must be a Platonist for whom the universe of "actual" existence is but an arbitrary locus in the eternal, the substance of his reflections on universals is to tie them indissolubly to the flux of things. The supposition of universals with no reference to the flux is as meaningless as the supposition of a flux which does not embody universals (2.330).

But there is no doubt Peirce was not always faithful to his best insights into this matter. Most puzzling is his philandering with the idea of final causes, so that he sometimes thinks of laws and ideas as operative and efficient. Thus, he declares that when an idea

is communicated "it had caused something to happen in your brain" (1.213); that while heredity is a law and not a force, it "like other laws doubtless avails itself of forces" (1.215); and that "every general idea has more or less power of working itself out into facts" (2.149). These expressions may be simply metaphors. It is, however, a metaphor which strangely persists in much of the discussion of Thirdness, and must make Peirce's readers question the integrity of his realism. For to endow laws with efficacy is to particularize them into things, and is an invitation to the nominalist to ask his unanswerable "When? Where?" Nor is the reader's doubt allayed when Peirce permits himself to say that the number system is our own creation, and that mathematical reasoning holds simply because it deals only with the creations of the mind (1.149, 2.192). It is a high price, even for fallibilism, to explain the unusual logical certainty of mathematics by regarding its subject-matter to be nothing more than an entertaining convention.

3.

Peirce was justly proud of his having been brought up in the laboratory and of so having acquired the experimental frame of mind when still young. And indeed his best developed writings are the contributions to the theory of deduction and to the analysis of probable reasoning.

The substance of his discussions of scientific method centers around his perception that science is more sure of the correctness of the general procedures it employs than it is of any specific result achieved by them. Those procedures involve the specific application of a self-corrective hypothetico-deductive method, and Peirce was convinced of the universal applicability of that method. He was eager to defend it against all comers, like a knight his fair lady whom he has chosen from all the world. He could, therefore, consistently attack the positivists for their doctrine that all hypotheses must be capable of direct verification (2.511n), and also declare that the full meaning of a proposition is to be found in what it prescribes us to do to gain perceptual experience (2.330). Even without waiting for his complete writings on pragmatism, which will appear in later volumes, it is possible to form a fair estimate of the studies on scientific method contained in these volumes.

Peirce made much of the distinction between explicative and

ampliative reasoning. Explicative reasoning is either *necessary* or *probable*, ampliative reasoning either *inductive* or *presumptive*. We have already considered the main features of the first, and shall return below to a more careful analysis of probable inferences. What is ampliative reasoning? In general, says Peirce, it is such that "the facts summed up in the conclusion are not among those stated in the premisses. They are different facts, as when one sees that the tide rises *m* times and concludes that it will rise the next time. These are the only inferences which increase our real knowledge, however useful the others may be" (2.680). But let us see what its species are.

Induction consists in inferring the constitution of a class from the constitution of samples drawn from it. We determine by induction the nature of a cargo of wheat through judicious sampling; this process determines for us, by empirical means, the ratio with which certain characters occur. *Presumptive reasoning*, on the other hand (also called abduction, retroduction, hypothesis), consists in inferring an explanation, cause, or hypothesis from some fact which can be taken as a consequence of the hypothesis. Presumptive reasoning is therefore the provisional adoption of a hypothesis, because every possible consequence of it is capable of verification, so that by pursuing this method its disagreements with facts will be revealed (1.68). The difference between induction and presumption is that the former infers the existence of phenomena homogeneous to those already observed, while in the latter something heterogeneous to that already observed is concluded, even something impossible for us to observe directly (2.640).

Now Peirce believes that these forms of inference are different from explicative probable reasoning, which he also calls statistical deduction. For the latter is probable, as we shall see more fully below, in the sense that though in a particular case its conclusion is false, yet in the long run and with a definite ratio, *similar* conclusions would be approximately true. But induction, for example, is probable only in the sense that though it may give a false conclusion, yet in the long run where the process is used, a *different* but approximately true conclusion would be drawn (2. 703). Thus, in probable reasoning the predicted conclusion is *verified* in the long run in a definite ratio of all the cases; in ampliative reasoning, the predicted conclusion will be *changed* in a good proportion of the cases, but will become approximately correct in the long run (2.709).

But it can be shown that the distinction between statistical deduction and the species of ampliative reasoning is only a *psychological* one, and refers simply to the various degrees of completeness our knowledge may possess. The distinction does not touch the *logical* question as to the *validity* of these forms of inference. For consider: If we reason at all, we must reason from premisses, and if we reason validly we do so because the conclusion is obtained in accordance with the leading principle or canon of the argument. When Peirce asks how it is possible that a man can observe one fact and then pronounce judgment concerning another fact not involved in the first, and when he then adds that such reasoning has no definite probability (2.690), he is surely inconsistent with his own analysis If the inference is made in virtue of some leading principle, it *does* have some degree of probability, namely, the relative truth-frequency associated with that principle. If the "inference" does not involve a leading principle, *no reasoning* takes place in any usual sense of the word. Thus, I sample a bag of beans and find half the sample is white. If I *know* nothing concerning the representative nature of the sample, I can *conclude* nothing about the color of the beans in the bag, although I may make a guess, consider my guess a hypothesis, and then proceed to test the hypothesis in the usual way. But if I *do* know that samples drawn in the way I drew this one *are* representative of the beans in the bag, I can conclude something about the constitution of the bag by statistical deduction; and the argument is probable in the sense to be defined presently. Ampliative inference is thus the experimental process by which we determine approximately the numerical values of the relative truth-frequencies involved. But as Peirce himself admits, its species are inverse forms of deductive probable inference, and their *validity* (the only relevant question in logic) is the validity of statistical deductions (2.511, 2.718).

We need discuss therefore only the theory of deductive probability, which Peirce adopted from Venn and developed in an original way.[8] The following are its essential features: (1) Probable inference, like all inference, depends for its validity upon determinate

8. A rigorously systematic discussion of the theory was never given by Peirce. It has been left for German students (Von Mises, Reichenbach, Tornier) to discover independently the fundamental principles of the truth-frequency theory.

relations between *propositions.* (2) No proposition is probable intrinsically, but only with respect to other propositions which are evidence for it. Probability is thus a *relation* between propositions. (3) Whether a proposition has or has not a degree of probability on definite evidence, does not depend on the state of mind of the person entertaining the proposition. Psychology is as little (or as much) relevant in probable inference as it is in necessary inference. (4) An inference is probable in so far as it belongs to a *class* of inferences in which the frequency of the conclusions being true is a determinate ratio of the frequency of the premisses being true. Probability is the *limiting value* of the fraction whose numerator is the number of times both antecedent and consequent are true and whose denominator is the number of times the antecedent is true, as the number increases *without limit.* Hence probability is not, as on Keynes' view, an unanalyzable concept. (5) The same proposition may have different degrees of probability in accordance with the sort of evidence which is marshalled in its support. But the relevance of the evidence can not be determined on formal grounds alone.

It is clear that since probability is the limit of an endless series, we can never assert categorically the value of a probability. Hence the assertion that such and such is the probability of a proposition is always a *hypothesis,* always subject to correction And hence, also, the sole business of the experimental sciences, since we can not exhaustively study all of nature, is to carry on *sampling operations* which will indicate approximately the values of different probabilities.

The question remains, therefore, under what conditions the process of sampling will lead us to an approximately true knowledge of the constitution of a field. Peirce prepares his reader by first rejecting *in toto* all theories of probability which are based upon the principle of equal distribution of ignorance; second (this is a corollary from the first) rejecting the use of inverse probabilities; and third, denying that the validity of sampling depends on any material assumptions such as the uniformity of nature (2.102, 2.749). According to him, sampling is a valid process whatever be the constitution of the universe, since its validity follows from the *manner in which the sampling is conducted.* And the following are the conditions under which sampling is a reliable process: first, the sub-

ject-matter must have a determinate nature; second, the sampling should be carried out without bias, the samples must be "fair"; and third, the characters whose distribution is investigated must be predesignated or predicted prior to drawing a sample, if that sample is to serve as evidence for any hypothesis concerning the constitution of the field. And Peirce concludes that while in any given case the process may lead to false results, it will, if pursued far enough, approximate more and more closely to the truth.

Now it is true that the method of sampling Peirce discusses is, in general outlines, the one pursued in the sciences. But isn't Peirce claiming more for the method than is warranted? Thus, the assertion that if samples are drawn "fairly," i.e., without bias, they will ultimately yield a "fair," i.e., representative, sample of the constitution of a field, is either a bald tautology, or is a *material* assumption to the effect that the specific mode of sampling is carried on in such a way that every variation in the field will turn up in our samples at some time or other. But if the amount of independent variety in the field should be indefinitely large (or even too large to handle with the methods at our disposal) we may never obtain representative samples no matter how, or how far, we carry on the process. *Some material* assumption, e.g., the existence of a limit in a rapidly converging series of variations, or the existence of a finite number of types or classes in nature, seems to be required if the process of "fair" sampling should yield representative ones. It is true that such a material assumption is itself a hypothesis, and must be evaluated in accordance with the usual canons. But this only goes to show that we can get *no absolute guarantee* in advance that any given method will work, no matter how trustworthy it may have shown itself to be.

I also can not escape the impression that in spite of Peirce's vehement denial of the principle of antecedent equiprobabilities, it returns to plague him. The process of sampling depends on the fact that more samples of a certain kind appear than others. But why do they? Because, the answer is, more samples of that kind occur in the field studied. But since samples not yet drawn exist only *potentially* in the field, it is legitimate to ask why there should be potentially more of one kind than of another. And the answer seems to be that if all the relevant characters in the field were combined in every permissible way, *and if all these permissible combinations were*

antecedently equally probable, then there would be more of one kind than of another.

Because of the sharp distinction Peirce makes between induction and presumption, he tends to obscure the identity of the processes employed, say, in evaluating the constitution of a cargo of wheat and in determining the correctness of the theory of gravitation.[9] This has as a consequence an unnecessary restriction of the frequency theory of probability, a restriction which is being removed by modern students. But already Royce showed that in evaluating a theory we are taking samples from all its logical consequences. There is therefore no good reason to doubt the universal applicability of the frequency theory of probability and the essential identity of scientific method in all departments of science.

4.

The editors of this edition deserve the congratulations of all students for their self-effacing and intelligent labor in bringing a semblance of order into a wilderness of manuscript. They have almost achieved what Peirce thought impossible for mortal man to do. Each volume has been supplied with very full indices, and in several cases with helpful explanatory notes. In the light of the enormity of the task and the excellence of the achievement, it may seem ungracious to find fault. But I think it is unfortunate that Peirce's larger manuscripts have been broken up and distributed throughout the volumes; this is especially true for the completed *Grand Logic*, the several chapters of the *Minute Logic*, and the *Lowell Lectures*. One may also question the judgment of the editors in neglecting to include in these early volumes certain outstanding papers to illustrate Peirce's doctrines: e.g., his review of Frazer's *Berkeley* for his views on nominalism, or his essay on the "Fixation of Belief" for his doctrine of fallibilism. Because of the topical arrangement of the papers, the historically interested reader will be able to estimate the extent to which Peirce's ideas underwent change only by a painful leafing of each volume. It is hoped the editors will manage to include a table in the final volume indicating where the component parts of

9. He declares he had committed a fundamental error in his Johns Hopkins essay on "Probable Inference," but he does not state explicitly what the error is (2.102).

the larger manuscripts are to be found, and possibly also the chronological sequence of the papers.

5.

The original speculative power and acuteness of Peirce's mind appear at their very best in the present two installments of the *Collected Papers*.[10] But space prohibits anything other than a brief indication of some of their striking contents.

Volume III consists entirely of previously published papers on what goes by the ill-fitting name of mathematical logic, and includes his great series of papers on the logic of relatives. Reading them consecutively one is impressed how steeped Peirce was in the mathematical researches of his day, and how the inquiries into the nature of generalized algebras by men like his father and Sylvester molded the course of his own thinking. There can be little question that Whitehead's *Universal Algebra* represents the culmination of some of Peirce's attempts in that direction. Many of the papers are now primarily of historical interest; and looking back upon them from the vantage ground of the elegant discussions in the *Principia*, they seem needlessly difficult simply because of the clumsy symbolism Peirce employed. And yet, in spite of the frequent incoherence of his statements, it is an inspiring experience to see him grappling heroically with novel ideas, conquering one only to attack another. These papers reveal that while Peirce strove mightily to achieve a philosophic system, he was essentially a man of great visions, but incapable temperamentally of organizing his ideas into unity and coherence. It goes without saying that he anticipated much that was independently discovered by others subsequently, e.g., the logistic thesis concerning the nature of mathematics, or the essential features of Poincaré's theory of applied geometry as a "convention." In addition, material more or less adequately treated in the preceding volumes of this edition, is restated and often amplified in both of the present volumes, so that a somewhat clearer light is thrown on such things as "fallibilism" in mathematics and the theory of signs. His comments on a variant of logical atomism are

10. *Collected Papers of Charles Sanders Peirce.* Edited by Charles Hartshorne and Paul Weiss. Cambridge: Harvard University Press. 1933. Vol. III, Exact Logic. Pp. xiv + 433. $5.00. Vol. IV, The Simplest Mathematics. Pp. x + 601. $6.00.

eminently worth reading, especially today. His defense of "infinitesimals" is provoking to say the least, even if not clear or convincing, and even if it seems to miss the point as to why the idea has been abandoned.

Volume IV consists chiefly of hitherto unpublished material on the foundation of logic and mathematics. Peirce anticipated the ideas expressed in Sheffer's stroke function, and the ideas as well as the use of the currently prominent matrix method. He made very full studies of the cardinality of classes, of the well-known contradiction, of orders of infinity, of linear algebras as instances of the general theory of relations; and his demonstrations have interesting, though sometimes uncritical, variations of the classic proofs. His judgment on the metaphysical importance of some of these studies seem now curiously unbalanced, e.g., the significance of the projective theory of distance; although anyone who has been exalted by that subject will surely forgive even an extended period of intellectual insobriety. His discussion of the logical priority of ordinal to cardinal number is particularly good and valuable, and ties up directly with the current *Grundlagenstreit* in mathematics. Also, little sermons on the value of logic, on how to read most profitably, on free will, on the issues of nominalism, and on the historical parallels between styles in architecture and types of logical theory, are to be found hidden away amidst technical discussions.

Peirce paid much attention to logical diagrams as instruments for the analysis of logical form, rather than as engines for calculation. And he regarded his Existential Graphs, a systematic scheme for representing geometrically any proposition however complex, as his *chef d'oeuvre.* A full account of it is included in Volume IV. But while the method is ingenious, and in many respects an improvement on the Euler and Venn diagrams, it is very clumsy; and it is difficult to attach to it the great importance which Peirce did. But perhaps the bearing of Existential Graphs upon Peirce's pragmaticism will become evident in later volumes.

All in all, Peirce's technical mastery of detail and his grasp of metaphysical issues as revealed in specific subject-matter are most impressively exhibited in this latest volume of the edition. It does not offer solutions to many pressing questions; it does not even ask some that one would like to ask. But it is a mine of suggestions to him who has the wit and patience to ferret them out.

6.

Of the volumes of the *Collected Papers* which have appeared thus far, the present one[11] is undoubtedly the richest in historical importance, in diversity of material, and in veins of thought which still remain to be mined. It contains the long awaited Pragmatism Lectures, and presents in available form Peirce's weightiest papers on general philosophy—those published in the *Journal of Speculative Philosophy*. I know of no better way to reach the heart of Peirce's doctrines than to read the important essays published in Book II of the present volume. The papers in Book III discuss Peirce's relations to the philosophy of common sense, and contain many of his own views under the label of critical common-sensism.

The Pragmatism Lectures repeat with simplifications and some elaborations doctrines already familiar from the first two volumes of the present edition: the phenomenology of the universal categories, the theory of signs, and the analysis of reasoning into its three main types. Peirce's strong idealistic leanings become very apparent here as elsewhere; and his views on reflective thought as an instrument for establishing general modes of behavior (thus subordinating logic to ethics), are brought out clearly. But on the whole the Lectures are disappointing: they are repetitious, they lack integration, and they contain many irrelevant though often interesting by-paths.

With the possible exception of the technical papers on strict logic, Peirce's writings on the theory of meaning seem to me to contain his most substantial contribution to philosophy. According to him, pragmatism maintains that nothing is in the understanding unless it is first in the senses, that nevertheless perceptual judgments always contain an element of generality or hypothesis, and that therefore such judgments are simply limiting cases of abductive reasoning and so always subject to correction. Ideas are to be clarified in terms of overt, public behavior of things, not in terms of private data of sense or self-luminous abstracta. In this insistance upon the intimate connection between sense and reason as well as upon the denotative reference of all intellectual conceptions, lie the

11. *Collected Papers of Charles Sanders Peirce*. Edited by Charles Hartshorne and Paul Weiss. Volume V, Pragmatism and Pragmaticism. Cambridge: Harvard University Press. 1934. Pp. xii + 455. $5.00.

strength and the difficulty of the pragmatic account of meaning. It eschews claims to infallible cognitive powers and yet avoids a stultifying scepticism. It must be admitted that the ambiguities which appear in Peirce's hitherto published writings on the theory of meaning are not ironed out in the papers now published for the first time. And yet if Peirce had been taken as the standard expositor of pragmatism, the often dreary and fruitless polemics on the nature of truth during the first two decades of the present century would have been avoided, with undoubted profit to the clarification of ideas in various departments of thought. For by pragmatism Peirce understood a method of making ideas clear, a branch of logic and scientific method, not a cosmology or a metaphysic. That is why, in spite of his penchant for a form of speculative idealism, he will remain a stimulus and a guide to all students of philosophy whose passion is to understand rather than to legislate.

Kant's influence upon contemporary positivism is a byword. It is interesting nevertheless to read Peirce's account of the indebtedness of his "proto-positivism" to the eighteenth-century thinker, and students of the history of ideas will find much in the present volume to repay their reading of it.

7.

Students of Peirce's writings who hope to find in the present volume[12] a carefully worked out metaphysics will be disappointed by its contents. Most of the papers included in it appeared in Peirce's life-time, and while some of the hitherto unpublished material helps fill out lacunæ, on the whole there is little in the volume which was not known before. Like the preceding volumes in this edition the present one is a curious mixture of grandiose cosmological speculation and penetrating analyses upon a variety of topics.

The volume begins bravely enough with a program for metaphysics of studying the most general features of reality. Peirce claims that the objects of metaphysical inquiry are open to observation in the same sense that the objects of science are, and he attributes the backward condition of metaphysics to the fact that its leading professors have been theologians. It is perhaps this pro-

12. *Collected Papers of Charles Sanders Peirce.* Edited by Charles Hartshorne and Paul Weiss. Vol. VI, Scientific Metaphysics. Cambridge. Harvard University Press. 1935. Pp. x + 462. $5.00.

gram and this claim that led the editors to choose the title of "Scientific Metaphysics" for the volume. However, while Peirce has many important things to say about the generic features of reality in his discussion of the categories, it is a Schellingian outlook which characterizes most of the volume. Peirce declares that "metaphysics has to account for the whole universe of being. It has, therefore, to do something like supposing a state of things in which that universe did not exist, and consider how it could have arisen." There follows the familiar tychistic-agapastic-synechism. Since Peirce believes that it is regularity which requires explanation, he tries to exhibit it as the product of evolution from the chance elements in the universe. His conclusion is that the only intelligible theory of nature is a form of objective idealism, according to which matter is effete mind, the habits of the latter becoming physical laws. An interesting feature in this cosmogeny is that not only the actual universe but also the Platonic realm of forms is evolutionary in origin.

The best things in the volume, to my mind, are the specific analyses Peirce makes of such ideas as necessity, chance, space, time, infinity, and continuity, and the additional light that is thrown on his views on probability and the nature of science. In his discussion of continuity, Peirce occasionally approaches the standpoint of contemporary intuitionism, although he does not work out the implications of his views. However, even in his analyses Peirce is dominated by his evolutionary cosmology, not always in the interest of clarity or cogency. Thus, while Peirce did yeoman service in pointing out the contingent factors in the laws of science, he seems to me to obscure the issues when he transforms "chance" into a substantial *agent*. And while his attack on necessitarianism is impressive, I do not find that he offers any respectable evidence for his view that the "order of nature" is changing, or that these changes are brought about through the operation of a primordial "mind." Again, his ideas on space and time, when I can understand them, seem to me very naive; and his conclusion that legal corporations have a "real" personality illustrates the tricks that words will play upon minds that should know better.

It is to be hoped that the remaining volumes of the *Collected Papers* will be published in short order. But enough of Peirce's work has now appeared to suggest the conclusion that a consistent

system of ideas can not be forced upon him. He said different and incompatible things at different times, and there seems no way of reconciling his period of forthright naturalism with the anthropomorphic idealism of other portions of his writings. Peirce characterized himself justly when he declared he was a "mere table of contents, so abstract, a very snarl of twine."

5

Charles S. Peirce, Pioneer of Modern Empiricism

No ACCOUNT of the development of contemporary empiricim is adequate which neglects the writings and the influence of Charles Peirce. Although he is not easily pigeon-holed and can not be claimed as the exclusive property of any school or movement, it is appropriate that the hundredth anniversary of his birth should be commemorated at this Congress. For the movement of which it is a manifestation is engaged in a coöperative, intensive cultivation of the methods of the sciences with the help of the most advanced tools of modern logic; and Peirce's intellectual career was also a single-minded devotion to that task. It is fitting, also, that his birth-year be celebrated at Harvard by an international congress. For although he was denied the privilege of teaching at this university, much of his influence was propagated by William James and Josiah Royce, two of its great teachers; and however much he may have suffered from neglect during his life-time, his work surely

merits recognition from a movement not confined by national bounds. It is characteristic of the best established sciences that though individuals may pursue researches in them independently of one another, the conclusions reached tend to support each other and to converge toward a common stream of sound beliefs; and such convergence is indeed the sole identifiable warrant for the confidence that some measure of the truth has been attained. It is therefore a happy sign that so many of the central ideas of the present movement have been independently developed on both sides of the Atlantic. One is not minimizing the contributions of the Vienna Circle in pointing out that many of its recent views have been taken for granted for some time by American colleagues, largely because the latter have come to intellectual maturity under the influence of Peirce.

It may nonetheless impress some as a paradox to count Peirce among the formative influences upon participants of this Congress. If, as Professor Frank recently stated, Mach is one of the spiritual ancestors of the unity of science movement and "the real master of the Vienna Circle," the Peirce who tried to construct a metaphysical system in the grand manner, and to provide a framework adequate for the results of the special sciences for a long time to come, seems definitely out of place in this company. He carried on a life-long polemic against positivisms of the type of Comte and Pearson; and though he had a high regard for some of the work of Mach, he expressed in no uncertain terms his condemnation of what he regarded as Mach's sensationalistic, nominalistic empiricism.[1] He confessed that he owed much to Schelling, and his architectonic cosmogony is admittedly a species of absolute idealism. He was a diligent student of the history of philosophy, he was brought up on Kant and claimed that at one time he knew the first *Critique* by heart, and he had a profound admiration for the scholastics.

But although Peirce had an abiding passion for system-building, his most vigorous efforts did not go into speculative metaphysics; and in any case the influence of his metaphysical writings has been practically nil—whether because only inadequate outlines of his grandiose system appeared during his life-time, or because other tendencies which Peirce initiated swamped the effects they may have

1. Cf. Peirce's review of Mach's "The Science of Mechanics," in *The Nation*, Vol. 57, 1893.

had otherwise. Indeed, seldom have severer judgments upon the claims and methods of traditional metaphysics been made than those to be found in his writings. He regarded the intrusion of metaphysical speculation into science as a hindrance to free inquiry, and distrusted an enterprise which has failed to develop methods of coöperative research among its members. His sharp criticism of Mach, though in my opinion unjust and not free from misunderstanding, were directed against what he took to be an attempt to legislate on metaphysical grounds the objects of knowledge, the limits of science, and the possible forms of physical theory.[2] But however this may be, it would be a gross misrepresentation of Peirce's views to regard them as minor variants of traditional philosophic doctrines. Peirce is the nearest thing to Leibniz this country has produced, and like the latter was able to combine a speculative bent of a high order of talent and ingenuity with a power of sober analysis and sense for concrete fact. His importance in the history of recent thought is due to his contributions to logic and mathematics, and to the stimulating effect of his first-hand knowledge of the theoretical and experimental sciences upon the study of scientific methods.

"From the moment when I could think at all," he wrote in 1897, "until

2. Thus he declared in the above review. "Sir Isaac Newton formulated the three laws of motion which stand today in all the text books The first, due to Galileo, is that a body left to itself continues for ever to describe equal spaces in equal times on one straight line. The third, Newton's own achievement in great measure, the law of action and reaction, is that one body cannot be drawn back without other bodies on the same line being drawn forward to balance it. Now Newton, with his incomparable clearness of apprehension, saw that the third law implies that spacial displacement is not merely relative, and further that, this being granted, the first law implies that temporal duration is not merely relative. Hence Newton drew the conclusion that there were such realities as Time and Space, and that they were something more than words expressive of relations between bodies and events This was a scientific conclusion, based upon sound probable reasoning from established facts. It was fortified by Foucault's pendulum experiment, which showed that the earth has an absolute motion of rotation equal to its motion relative to the fixed stars. Moreover, Gauss and others were led to ask whether it be precisely true that the three angles of a triangle sum up to two right angles, and to say that observation alone can decide this question. . . . But Mach will not let it go so. His metaphysics tells him that there is no such thing as absolute space and time, and consequently no such thing as absolute motion. The laws of motion must be revised in such a way that they shall *not* predict that result of Foucault's experiment which they did successfully predict, and that non-Euclidean geometry must be put aside on metaphysical grounds. Is not this making fact bend to theory?" *Op. cit.*, p. 252.

now, about forty years, I have been diligently and incessantly occupied with the study of methods of inquiry, both those which have been and are pursued and those which ought to be pursued. For ten years before this study began, I had been in training in the chemical laboratory. I was thoroughly grounded not only in all that was then known of physics and chemistry, but also in the way in which those who were successfully advancing knowledge proceeded. I have paid the most attention to the methods of the most exact sciences, have intimately communed with some of the greatest minds of our times in physical science, and have myself made positive contributions—none of them of any very great importance, perhaps—in mathematics, gravitation, optics, chemistry, astronomy, etc. I am saturated, through and through, with the spirit of the physical sciences."[3]

It is unnecessary on this occasion to speak of Peirce's contributions to formal logic (e.g., his improvements upon Boole's work, his development of the logic of relations, etc.), for these have been recognized long ago; and it is a safe conjecture that his technical papers on these subjects are now primarily of historical interest. It is worth pointing out, nevertheless, that for Peirce formal logic was simply one division of the general theory of signs. An adequate logical theory, according to him, must take into account the complicated properties and functions of signs in inquiry, and even the rules of formal logic were regarded by him as intimately related to the habits of action generated in the course of successful inquiry. Thus, the alleged facts of "consciousness" were dismissed by him as totally irrelevent to the question of the validity of the laws of logic; for even if consciousness were annulled it would still remain true, he said, "that such and such a habit of determining one virtual store of knowledge by another will result in the concentration of action so as to bring about definite ends"—to which he added that rationality consists in the fact that the rational being will act so as to attain certain ends.[4] Accordingly, Peirce was one of the most pronounced foes of attempts to base logic upon inner feelings of certitude or other facts of individual psychology, and his conception of the nature and function of formal logic is incompatible with interpretations of inquiry in terms of a "mentalistic" theory of thinking.

Peirce's distinctive contributions to logic as the general theory

3. Charles S. Peirce, *Collected Papers*, 1.3. All references, unless otherwise specified, are to volume and paragraph of this edition of Peirce's writings.

4. 2.66.

of signs center around his pragmatism, his critical common-sensism, and his fallibilism. By far the best known is his pragmatic maxim, proposed as a method for clarifying ideas, eliminating specious problems, and unmasking mystification and obscurantism hiding under the cloak of apparent profundity.[5] In one form or another his proposal was adopted by a number of distinguished thinkers, for example, in this country by William James and John Dewey, so that today it is almost a commonplace. Peirce's own formulation of the pragmatic maxim leaves much to be desired in the way of explicitness and clarity; and more recent formulations, such as those by Professor Carnap and others, have the same general intent but superior precision. I nevertheless venture two general remarks on the Peircean version of pragmatism which, though obvious, merit attention.

The pragmatic maxim was intended as a guiding principle of analysis. It was offered to philosophers in order to bring to an end disputes which no observation of facts could settle because they involved terms with no definite meaning. It was directed at the Cartesian doctrine of clear and distinct ideas, which found the terminus of analysis in vague abstractions claimed to be grasped intuitively, as well as at the common tendency to convert types of behaviors into unknowable agencies controlling the flux of events. Above all, it pointed to the fact that the "meanings" of terms and statements relevant in inquiry consist in their being *used* in determinate and *overt* ways. Pragmatism, to employ Peircean language, was thus a proposal to understand general terms in terms of their concrete application, rather than vice versa. "We should hardly find today a man of Kirchhoff's rank in science," he wrote in 1903, "saying that we know exactly what energy *does* but what energy *is* we do not know in the least. For the answer would be that energy being a term in a dynamical equation, if we know how to apply that equation, we know thereby what energy is. . . ."[6] Now

5. His earliest version of it runs as follows. "Consider what effects, that might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception of the object." 5.402.

6. 5.207. Twenty-five years earlier Peirce wrote: "In how many profound treatises is not force spoken of as a 'mysterious entity,' which seems to be only a way of confessing that the author despairs of ever getting a clear notion of what the word means! In a recent admired work on *Analytical Mechanics* it is stated that we understand precisely the effect of force, but what force

my first point is that the pragmatic maxim should be construed as an invitation to analyze specific concepts in a definite way. This "definite way" places emphasis upon the contexts in which terms occur and upon the complex of relevant practices involved in their use. Pragmatism does not supply a formula which states once for all what *the* meaning of a statement is; for when its maxim is taken seriously, it excludes the assumption that the meaning of a sentence can be determined in isolation from the system of sentences in which it occurs, the rules governing its acceptance, and the kind of behavior associated with it. It is unfortunate that so much energy has gone into controversy about the wholesale meaning of "meaning," as if any formula could state with adequacy the multifarious ways in which that word is used, instead of being turned into analyses of the specific uses of terms in different contexts. Would it not be well to shelve the term "meaning" in philosophic discussion and talk instead of the way in which terms are *used*? Pragmatism does not constitute another ambitious "prolegomena for every future inquiry," and it would be a pity if, mistaken for such rather than taken as a fruitful leading principle of inquiry, it would meet the fate of the Cartesian, Lockian, and Kantian systems. Peirce himself instituted the sort of analysis of several specific concepts called for by his maxim, and Mach's work is still valuable primarily, it seems to me, because of the splendid examples of such analysis it carried through.

I have already skirted my second point. Peirce's version of pragmatism locates the intellectual purport of conceptions not in individual sensations or perceptions, but in the concrete habits or tendencies to action to which their acceptance leads. "The whole function of thought," he maintained, "is to produce habits of action; and that whatever there is connected with a thought, but irrelevant to its purpose, is an accretion to it but no part of it. If there be a unity among our sensations which has no reference to how we shall act on a given occasion, as when we listen to a piece of music, why we do not call that thinking. To develop its mean-

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itself is we do not understand! This is simply a self-contradiction. The idea which the word force excites in our minds has no other function than to affect our actions, and these actions can have no reference to force otherwise than through its effects. Consequently, if we know what the effects of force are, we are acquainted with every fact which is implied in saying that a force exists, and there is nothing more to know." 5.404.

ing, we have, therefore, simply to determine what habits it produces, for what a thing means is simply the habits it involves. Now, the identity of a habit depends on how it might lead us to act, not merely under such circumstances as are likely to arise, but under such as might possibly occur, no matter how improbable they may be. What the habit is depends on *when* and *how* it causes us to act."[7]

It is clear that this pronouncement is at least verbally different from the current version of the verifiability principle, according to which empirical propositions must be reducible to observation *statements* containing various sorts of observation *predicates*. Without intending to minimize the remarkable clarification which such a formulation has introduced or to question its superiority in point of precision over that given by Peirce, it seems to me, nevertheless, that an adequate empiricism can not afford to neglect Peirce's emphasis upon *habits of action* or to concentrate entirely upon the reduction of one set of statements to others. For in the first place, this emphasis makes evident the continuity of knowledge-getting with other organic activities. It involves the conception of inquiry as occurring in a definite context, instituted to settle specific doubts and problems, and terminating in an equally definite way with the establishment of smoothly-working habits of action which have mastered the difficulties for which the inquiry was initiated. Knowledge thus becomes identifiable as the product of overt behavior involving cooperative effort in a community of inquirers, rather than as the outcome of a purely subcutaneous mental activity. So conceived, knowledge is in fact frequently obtained, for the claim to possess it does not involve the preposterous requirement that the outcome of one inquiry be incorrigible by further inquiry, or that unless it carries the traits of absolute finality and all-inclusiveness the product of research is not knowledge. Peirce thus laid the foundation for a theory of inquiry which eschewed a mentalistic, introspective psychology, and he espoused a molar behaviorism at a time when it was regarded as fanciful speculation.

In the second place, Peirce's emphasis is fatal to the conception that science must be grounded upon an indubitable apprehension of simple elements, whether these be atomic-facts, sense-data, or essences, all pre-manufactured and neatly packaged; for it entails

7. 5.400.

the view that alleged "simples" must be contextually interpreted and that they are products of inquiry which has learned to isolate certain features of the environment as reliable clues to what is going on. It points to the fact that the adequate use of even the simplest observational terms of whatever type involves habits of manipulating objects in the environment, and that the established sense of even the most abstract terms of science involves reference at some point to gross, overt activities. Surely, no one would take seriously the professed acceptance of the physicalistic thesis of the reducibility of all terms to those occurring in every-day observation, if that acceptance did not go hand in hand with appropriate types of gross behavior associated with such observation terms. And the contemporary insistence upon methodological principles as resolutions and rules of language, fails in its objective to exhibit the logical articulation of science if it neglects the practical compulsions involved in the construction and manipulation of technological and laboratory apparatus.

At the risk of treading upon ground on which angels fear to step, I should also like to mention the elementary point that in terms of Peirce's emphasis neither terms nor statements can be regarded as *designating*, independently of the habits involved in their use. Consequently, "the meaning" of expressions is not to be sought in self-subsisting "facts," "essences," or other "designata," but must be construed in terms of the procedures associated with them in specific contexts. Some have suspected, perhaps unjustly, that the recently inaugurated discipline of semantics will open wide the door for the rehabilitation of Bolzano's *Saetze-an-Sich*, Meinong's *objectives*, Russell's *subsistents*, and allied conceptions of the referends of signs. Though such doctrines have had fruitful historical rôles, I think it would be a retrograde step if modern logical empiricism were to revive them in a new form; for the great strength and promise of the movement has been its interpretation of the abstract in terms of the concrete, and its resolute turning from speculations which have no ascertainable consequences in issues of observable fact. I can think of no better way to still these suspicions than by placing the study of semantics into a behavioral context, and by instituting an analysis of such key semantic terms as "designation" and "truth" as used in *specific* contexts, in order to reveal the modes of action they signify.

Peirce's critical common-sensism is integral to the pragmatic attitude as developed in the present paper. Though indebted to the Scotch common-sense realists for some of his ideas and language, Peirce had nothing in common with their dogmatic faith in self-evident truths or their attempets to establish a bedrock foundation for current theologies and social theories. His critical common-sensism was an elaboration of his fundamental insight that in any inquiry we must plunge into *medias res* with all the beliefs and prejudices we actually have, and that we can not, even in principle, engage in a universal Cartesian scepticism. Every inquiry uses and takes for granted many habitual modes of action as well as specific assumptions about the course of events, which in *that* inquiry function as indubitables; and many of these assumptions, largely because they are vague, have withstood the test of repeated experience, so that it would be idle to doubt them. The adequacy of specific beliefs and habits is constantly put into question; but there can be no intelligible scepticism concerning our crude experience as a whole, since the formulation of every specific doubt requires distinctions themselves significant only in terms of the procedures of the common-day world. It is not possible, therefore, to dispense with the vague indubitables of every-day experience. Habitual behavior can not be "justified" by any theory of logic or of the universe, and it can not be "clarified" by trying to exhibit it as a complex of psychological or ontological simples, since every attempt to do so must be brought to the test in the work-a-day world of gross, vague distinctions. The justification and clarification can be brought about only by showing what the consequences of such behavior are, and that it is adequate to the situation at hand—it being noted that the consideration of consequences and adequacy leads finally to the same sort of gross behavior which furnished the occasion for doubt.

Peirce claimed no infallibility for the beliefs of every-day experience, and indeed one of the cardinal tenets of his thought was a universal fallibilism. Peirce's fallibilism is a consequence of his regarding the method of science as the most successful yet devised for achieving stable beliefs and reliable conclusions; it has nothing to do with the malicious scepticism which rejects science on the ground that its conclusions are after all not established as being beyond the possibility of error, only to invoke a special set of impera-

tives as indubitable objects of human endeavor. Peirce noted that the conclusion of no scientific inquiry is exempt from revision and correction, that scientists feel surer of their general logic of procedure than of any particular conclusions reached by it, and that the method of science is self-corrective, both as to its own specific features and the specific conclusions gained with it. These insights dovetail neatly with what has already been said concerning the importance of construing concepts and statements in terms of the procedures associated with them; for it is a simple corollary from this that the conclusions of science must also be understood in terms of the methods used to establish them, so that the reliability of the conclusions must be a function, first and last, of the character of the methods. That is why Peirce found unacceptable theories of atomic facts and sensory simples as indubitable objects of knowledge, or the pretensions of philosophers to map the limits and possible objects of science—for each of these illustrates a dogmatism which blocks the road to inquiry, and to block this road was for Peirce the least forgivable and most dangerous intellectual vice.

Peirce was one of the earliest proponents of the frequency interpretation of probability statements, and though contemporary formulations of that view are superior to his, what he has to say on the general theme may still be read with profit. As is well known, there is still disagreement among competent students as to the scope of the frequency theory of probability and its relation to the procedures of induction. Peirce's views on this point, though not free from difficulties, suggest promising lines of inquiry.

"The true guarantee of the validity of induction," he declared, "is that it is a method of reaching conclusions which, if it be persisted in long enough, will assuredly correct any error concerning future experience into which it may temporarily lead us. . . . It may be conceived, and often is conceived, that induction leads a probability to its conclusion. Now that is not the way induction leads to the truth. It lends no definite probability to its conclusion. It is nonsense to talk of the probability of a law, as if we could pick universes out of a grab-bag and find in what proportion of them the law held good. . . . The justification for believing that an experimental theory which has been subjected to a number of experimental tests will be in the near future sustained as well by further such tests as it has hitherto been, is that by steadily pursuing that method we must in the long run find out how the matter really stands. The reason that we must do so is that our theory, if it be admissible even as a

theory, simply consists in supposing that such experiments will in the long run have results of a certain character."[8]

Peirce's writings on induction are not uniformly clear, but in the main they are consistent with his insistence upon the character of the *method* used to test them as providing the only identifiable ground for regarding the theories of science as reliable.

Toward the end of his life Peirce enumerated a number of tenets which he regarded as fundamental to his version of pragmatism, and which set him off from other thinkers. The list included the following: the denial of necessitarianism; the rejection of any "consciousness" different from a visceral or other external sensation; the acceptance of the notion of infinity as significant; the acknowledgment that there are real habits which would produce effects under circumstances that may not happen to get actualized; and the insistence upon interpreting all hypostatic abstraction in terms of what they would or might come to in the concrete.[9] It is dangerous, I know, to try to force a fluid and developing movement into the framework of a set of rubrics devised for another purpose; nevertheless, it seems to me that on these points contemporary logical empiricism differs from Peirce only in relative emphasis and mode of presentation, and stands closer to him than did most of his own contemporaries. It is not improper, therefore, to claim him as a spiritual ancestor of the movement which this congress represents, and as an important formative influence upon the development of at least its American participants.

Peirce once evaluated his own work as follows: "Just as there are many fogies—old and young—who with idle conservatism dispute the value of my work, so, unless the whole congregation of logicians experiences a regeneration, I expect the day will come when another generation of old and young fogies will be equally indisposed to admit that there is any corner of the whole field that I have not turned up, and put into the right condition."[10] The density of old and young fogies among logical empiricists is I think vanishingly small, and there is probably no one among its members and sympathizers who would accept Peirce *in toto*, or who would not acknowledge him to have been mistaken on a number of funda-

8. 2.769, 2.780, 5.170.
9. 6.485.
10. 6.319.

mental points, such as his naive conceptions of absolute space, his curious apriori deduction of the types of actual physical forces, or his failure to note the regulative and definitory function of a number of scientific principles. We can nevertheless honor him with clear conscience for what he achieved of lasting value. Were he still among us he would surely have endorsed the happy marriage of the cultivation of logic and the empirical temper which distinguishes this movement, and he would have joined hands with us in furthering the quest for and the understanding of progressively more adequate tools of inquiry.

6

Dewey's Theory of Natural Science

I.

THERE IS A CURIOUS and distressing paradox associated with the growth of modern natural science. No one doubts that the expansion of experimental techniques in conjunction with the development of mathematically formulated theory has given us unprecedented intellectual and practical mastery over many sectors of nature. Indeed, the maxim that knowledge is power, is widely acknowledged as a truth that has become an almost painful platitude. Nevertheless, scientific theory is frequently so construed that instead of rendering the universe more intelligible and making men feel more at home in it, both the constitution of nature and man's place in it have become more puzzling and mysterious.

Two interpretations of modern science have had an especially wide currency. According to one, the discoveries of natural science, especially of physics, make it impossible to suppose that the familiar

aspects of things encountered in every-day experience have a genuine place in the objective order of nature, or that the qualities manifested in our ordinary commerce with the world are anything but illusory appearances. According to the other, the conceptions of theoretical science are mere fictions, and at best only convenient practical devices; they do not express the true characters of things, and they are without relevance for matters of intimate human concern. On either view, incoherent answers await questions concerning the obvious efficacy of scientific knowledge in controlling the course of familiar events, or concerning the relation of the human scene to the rest of nature. On either view, fixed limits are imputed to the scope of scientific method, and in consequence large areas of human experience are held to be inherently incapable of fruitful exploration by responsible scientific inquiry. In short, instead of being recognized as an agent for liberating and redirecting human energies, modern science is viewed in many quarters with anxious dread or complacent indifference.

The factors responsible for this paradoxical state of affairs are to be found partly in the historical circumstances under which modern science has developed, and partly in the inherent character of modern natural science. Natural as well as social sciences have been cultivated in an environment in which powerful vested interests have been hostile to the pursuit of free inquiry. For many centuries, prevalent conceptions of social and political organization, no less than entrenched beliefs on moral and religious subjects, have been coupled with traditional assumptions concerning the mechanisms of physical nature. Accordingly, the intellectual progress that successfully challenged the latter also became a constant threat to hallowed social ideals and deeply laid religious convictions. To be sure, established churches and other institutions eventually learned how to square their theological, social and moral creeds with the latest findings of physical and even biological science. And what more effective means for doing just this could have been devised than that of so construing scientific findings that they are merely convenient formulations of techniques of control, incapable of ever rendering some assumed "inner nature" of things? Even so, a strong emotional and overt resistance still persists to what is widely felt to be the intrusion of scientific methods into the discussion of human affairs.

But there are other reasons, more intrinsic to the nature of modern theoretical science, for the paradoxical philosophies that have trailed its developments. Modern science is not primarily concerned with the uses which things have in specifically human contexts, but rather with the invariant conditions under which events occur and with the mutual interrelations of things. In consequence, theoretical physics operates with distinctions that are both highly abstract and apparently incongruous with notions employed in habitual experience. In point of fact, the objects postulated in modern physics, such as atoms and electrons, possess by their very definition few if any of the qualities that identify and mark off the familiar things of human life. For example, neither colors, nor sounds, nor odors, nor even determinate positions and shapes are attributed to the fundamental particles of current sub-atomic theories. However, if physics is assumed to disclose the ultimate and exclusive characters of things, there is an obvious discrepancy between what the sciences apparently teach and what is found in gross experience. How tempting and seemingly cogent, therefore, is the explanation of this discrepancy which views the distinctions and principles in terms of which we order our daily lives as belonging merely to a realm of subjective appearance!

This conclusion, moreover, is often supported by another line of argument. The physics and physiology of perception show that the qualities commonly attributed to things are in fact manifested only under special conditions, among which must be counted the presence of biological organisms. It seems plain, therefore, that the directly apprehended qualities are not traits which things possess absolutely and independently of their interaction with organic bodies. And since such qualities do not belong to the "world of physics," it is concluded that they do not form part of the objective order of nature. In brief, contrary to the naive realistic view, things are not what they seem. On the other hand, every actual inquiry into the causal patterns of nature takes its point of departure from immediately experienced qualitative events, and it also terminates in such an experience. For it is in the precarious existence of qualitative events that the sciences find their problems; and it is in the agreement of their occurrence with assumed laws that evidence for the latter is obtained. But if the qualities encountered in common experience are held to belong exclusively to a realm of subjective

appearance, while the mechanisms discovered by physics are taken to constitute the true reality, then science is indeed at war with itself. As one typical contemporary reading of the import of natural science states the general outcome, naive realism leads to physics, but if physics is true then naive realism is false. On this analysis, therefore, one is left with the choice of either accepting natural science but rejecting as basically illusory the things that constitute men's most familiar and valued experiences, or accepting the objective character of the common-sense view of things but denying the validity and relevance of modern science for matters of prime human concern.

2.

It is this alleged impasse of modern science, so paradoxical in its formulation and so devastating in its practical consequences, which has been at the focus of Dewey's attention during his long philosophical career. He has consistently maintained that this impasse is generated by misconceptions concerning the relation of the objects and distinctions of physical science to the things of ordinary experience. And he has in fact identified as one of the outstanding issues of modern philosophy the clarification of just this problem.

Dewey has been intensely concerned with the problem, because he recognized early in his career that mistaken notions about the status of physical objects stand in the way of successfully exploiting the potentialities of modern science for the enrichment of human life. In particular, he has noted repeatedly and with untiring zeal the fatal import of the traditional dualistic interpretation of science for the other paramount issue of modern philosophy—the integration of men's beliefs about the world as derived from natural science, with the principles in terms of which men direct and evaluate their private and public conduct. An interpretation of the nature of scientific objects which requires either the wholesale rejection of ordinary experience as illusory, or the condemnation of theoretical science as mere convenient fiction, makes impertinent the findings of natural science to the problems of human society. Such an interpretation effectively deprives the tried logic of scientific method of any authority in the treatment of morality, and kills in the bud the promise implicit in the scientific enterprise of freeing men from the bondage of frustrating custom.

Dewey's ultimate concern with the technical problems of the theory of knowledge is thus moral in intent. However, he has not been content with simply identifying the crucial issues of modern philosophy; and he has never discussed them in merely moralistic terms. A long lifetime of sustained effort has gone into his attempt to solve these problems, and to solve them in a technically competent manner. His analysis of the status of scientific objects is indeed not written for the man who reads as he runs, and is inextricably linked with details in his often difficult account of the nature of knowledge. Moreover, much of his discussion is carried on in the midst of a far-flung polemic against what he believes are outmoded views on the subject. For in his attempt to resolve the alleged impasse of modern science, an essential feature of Dewey's strategy consists in showing that the problem vanishes and becomes unintelligible, when once the traditional preconceptions which generate it are successfully challenged.

Dewey's analysis of the relation of scientific objects to matters of direct experience is rooted in one firm assumption which he does not always make fully explicit. The premise from which he operates is that distinctions must be understood in terms of the concrete uses for which they are devised, and that in particular scientific ideas must be construed in terms of their identifiable functions in the context of inquiry. This assumption is unavoidable in any responsible analysis, and is in any case decisive. Unless scientific conceptions are construed in agreement with it, any proposed interpretation of scientific discourse will be arbitrary; it will be based on preconceptions derived from some tacit philosophical commitments, rather than on the actual operative meaning of the language of science.

It was Einstein who advised students of the methods of theoretical physics to concern themselves, not with accounts scientists happen to give of what they do, but with their actual procedures and achievements. Long before this advice was given, and long before it became fashionable to preach the virtues of functionalist or contextualist analysis, Dewey was practicing it with energy and a considerable measure of consistency. It is from this standpoint that he has conducted intellectual war into the camps of traditional philosophies of science; and by adopting that approach he has saved himself from much futility and avoided some serious errors. It has

prevented him from regarding scientific theory as merely a condensed transcript of the immediate content of experience, as well as from attempting the impossible task of translating theoretical statements into statements about directly apprehended sensory qualities. It has led him to emphasize the continuity of refined scientific procedures with modes of solving problems on more primitive levels of behavior, and to recognize in consequence that the conception of scientific theory as a system of mere fiction makes incomprehensible the whole practical life of man. It has enabled him to see that the adequacy or validity of ideas is not warranted by their supposed derivation from materials of sense, but rather by the consequences of their use. And above all, it has served him as the means for showing that science is not a disclosure of a reality superior to and incompatible with the things of ordinary experience.

3.

The central thesis of Dewey's theory of science is that it does not disclose realms of being antithetical to the familiar things of life, simply because scientific objects are formulations of complex relations of dependence between things in gross experience. More specifically, the constructions of theoretical physics are viewed by him as intellectual means for organizing the discontinuous occurrences of directly experienced qualities, as ways of thinking about matters in gross experience in order to obtain some measure of control over their histories.

It follows directly from this thesis that scientific discoveries concerning the conditions of occurrences of things in ordinary experience cannot possibly impugn the objective reality of the latter. Why should the fact, for example, that the occurrence of heat and cold is contingent upon certain distributions of molecular energies, constitute ground for denying that the sun is hot or that snow is cold? And why should the further fact that in the absence of definite physiological conditions these qualities are not manifested, count as reason for denying that heat and cold are objective features of those existential situations in which both the physical and physiological conditions are realized?

To be sure, physics is not concerned with the physiological conditions for the occurrence of various qualities; for physics seeks to find relational orders of dependence that are invariant for all per-

cipient organisms, and that are independent of the latter's presence or absence. But what is canonical for physics is not therefore a measure of objective existence; and only an arbitrary preference, rooted in an influential intellectual tradition, will assign exclusive reality to invariant relational orders. In point of fact, even the properties which physics ascribes to its objects, such as mass, are manifested only under certain contingent if pervasive conditions. Were every property which is relational in this sense denied a place in the objective order of nature, physics too should be regarded as dealing only with what is subjective. The crux of the matter is that if only absolutely invariant universal patterns of relations are assumed to be genuinely objective, nothing that is individual, specific, and limited can have any place in objective nature. But those who adopt this criterion are then faced with the outrageous paradox of having relations without terms to be related as the sole furniture of genuine reality. Is the notion of a cat with a grin but no body a more fantastic conception of the way of things?

Dewey's account of scientific objects is thus accompanied by a reaffirmation of the claims of gross experience. But the naive realism he defends is free from the dogmatic naiveté so frequently associated with philosophies of common sense. He emphatically includes the various qualities of ordinary experience among the ultimate furniture of the world. But he does not assume that the immediate apprehension of qualities constitutes knowledge of them. Knowledge for Dewey is always the terminus of *inquiry*, and involves the establishment of relations of dependence between what is thus directly experienced and what is not. What these relations are, however, is not to be settled by intuition or authority, whether the problem under consideration involves issues of physics, private morality or public policy. It is pre-eminently a matter requiring reflective thought or experimental inquiry.

Indeed, it is precisely in this context that the objects of science manifest their distinctive functions. It is in situations where one seeks to discover the conditions upon which the occurrence of immediate qualities depends, that scientific objects serve as general schema for analyzing things of ordinary experience in terms of their systematic relations to other events and potentialities in nature. Accordingly, the postulated objects of theoretical physics are not things which in turn require to be sensed or directly experienced. When viewed

in terms of their status identifiable in inquiry, they represent ways of conceiving and ordering what is capable of being sensed and experienced. No legitimate puzzle resides in the fact that the objects of physical theory do not possess the qualitative differentia of things of every-day experience. A mystifying puzzle does arise if the former are converted by some dialectical prestidigitation into another set of individuals which require to be directly encountered if they are to be adequately known.

4.

An impressive quantity of concrete evidence confirms Dewey's account of the status of scientific objects, but there are also difficulties which confront it. Both supporting evidence and difficulties must now be briefly examined.

It is beyond reasonable dispute that at least one function of any scientific theory is that of a generalized directive for treading the complex maze of events with which men are directly confronted in gross experience. For as has already been noted, every inquiry into empirical subject-matter is initiated and controlled by problems concerning matters that are encountered in such experience. And every proposed resolution of such problems, every theory which aims to specify the conditions upon which the occurrences generating these problems are contingent, must satisfy the minimal requirement of indicating factors to be found in gross experience through which those conditions can be identified. For otherwise problems are not solved, and theories are speculative fancies which cannot be checked by observational data. Accordingly, even when a theory postulates the existence of particles and processes which are quite unlike the materials of ordinary experience, the *raison d'etre* of the postulation is the enlarged and clarified understanding of the mutual relations between these latter that the postulation may help to achieve.

It would be superfluous to cite examples in which theoretical constructions do serve as guides for mastering men's environment—the full basis for this claim must be sought in the detailed history of the theoretical sciences and the technologies nourished by them. But apart from examples and history, the claim will be reinforced if attention is directed to some familiar features of physical theory and to certain circumstances of the use of such theory in inquiry.

A striking trait of theories in the natural sciences, and especially of mathematical physics, is the absence of any mention in them of specific places and dates or of individual objects and events. In consequence, these theories do not describe anything actually existing in any specified sector of nature. How then does such a theory acquire a specific import for specific problems dealing with specific spatio-temporal events, for example for a problem concerning the occurrence of a solar eclipse at some place on the earth's surface? The answer is well known. A theory can help resolve a problem concerning definite spatio-temporal occurrences, only if it is supplemented by special information about local configurations of events and objects *which the theory itself indicates as being relevant.* For solving the problem about the occurrence of an eclipse, for example, the special information needed includes the relative positions and diameters of the sun, moon, and the earth at a certain time. But the need for such supplementary information makes plain that a theory is a generalized formula for resolving a broad class of problems, and that one important function of the formula is to indicate *just what* specific data must be secured about an actual situation if a concrete instance of the class of problems is to be solved. Accordingly, although a theory is not itself a description of what happens to exist, it is at the very least a generalized plan for guiding the direction of overt observation and experiment on what does exist.

There is another feature of physical theory that deserves notice. The fundamental assumptions of many comprehensive theories are usually stated for so-called "pure" or "ideal" cases, and more generally theoretical statements are frequently formulated in terms of "limiting" or "ideal" concepts. Such pure cases are rarely if ever encountered in experience; and limiting concepts, sometimes as a consequence of their mode of definition, cannot be taken as descriptive characterizations of anything that is experimentally identifiable. For example, no completely isolated physical systems occur in nature, though at least one of Newton's axioms of motion is stated for just such a system; and notions such as that of instantaneous velocity or perfect elasticity are widely employed in mechanics even though no actual bodies can be directly subsumed under them. Nevertheless, pure cases and ideal concepts have significant uses in inquiry: through their use it is possible to state with maximum economy and com-

prehensive generality what are the relevant factors in qualitatively diverse materials upon which the course of events depends, and thereby to state the mutual interrelations of apparently independent processes. However, and this is the essential point, the systematic analysis that is achieved through the use of ideal concepts can be carried through only if these concepts are associated with explicit or tacit rules of overt procedure for handling materials of gross experience. For since ideal concepts are not directly applicable to the objects of ordinary experience, these latter must be "cooked" and worked over if they are to be subsumed as "instances" under such concepts. A theory which uses the notion of perfect elasticity, for example, must be coupled with a set of directions that specify, even if only vaguely, how bodies are to be classified and ordered with respect to their elastic properties; otherwise the theory swims in a void, and cannot serve to illumine anything actual. But if such rules are inevitable supplements to viable theories employing ideal concepts, those theories will always contain as an essential component directives for discriminating relevant features in concrete subject-matters. It is in part because these associated rules of procedure are frequently not formulated explicitly, that theories often seem so utterly remote from and even incompatible with what is encountered in direct experience. And it is in part because scientific theories are frequently interpreted simply on the basis of their *formal* structure, and in disregard of the tacit rules essential for their use in dealing with specific existential problems, that the function of theories as guiding principles for overt observation and experiment is often ignored.

5.

It has been customary in the analysis and interpretation of science to distinguish between two types of theory. One type, variously labelled as "macroscopic," "phenomenological," and "abstractive," is said to "abstract" determinate relations between macroscopic, directly identifiable objects, and to eschew all hypotheses which assume "hidden" and "unobserveable" mechanisms that operate "behind" the manifest phenomena. Newton's theory of gravitation, Fourier's theory of heat conduction, and classical thermodynamics, are the commonly cited examples of this type of physical theory; and Newton's famous dictum "Hypotheses non fingo" is generally

understood to mean that he declined to entertain theories of any other type. The second type of theory, variously called "microscopic," "hypothetical" and "physical," does by contrast explicitly postulate unobserveable particles, sub-microscopic processes, and mechanisms not open to direct observation, in terms of which the complex phenomena of gross experience are to be explained and understood. Familiar examples of this type of theory are the numerous atomic and electric theories of matter.

It is by no means a closed issue whether the distinction between these two types of theory is either important or even well-founded, though this question will not be pursued here any further. The relevance of this distinction for the present discussion is that it helps to make explicit the chief objection usually raised against the conception of scientific theory as being primarily an instrument for the conduct of inquiry. This conception is frequently acknowledged as illuminating and sound when it is stated for macroscopic theories. For in such theories no domain of events and individuals is postulated which is distinct from the events and objects encountered in ordinary experience; and accordingly, the scientific objects assumed by such theories are easily construed as structures of relations between the materials of gross experience. But the situation seems to be quite different in the case of microscopic theories. The impressive successes of atomic and electronic theories of matter in predicting and bringing into systematic order a wide variety of phenomena, have convinced a good fraction of contemporary scientists that the scientific objects postulated by theories of this type are more than systems of relations between familiar objects, and that on the contrary these scientific objects are concrete individuals, possessing spatio-temporal locations and participating in dynamic transactions with each other. Although it is generally admitted that these scientific objects lack most of the qualitative traits which mark off the familiar objects of experience, they are nevertheless believed to be physical constituents and parts of these latter. In brief, the scientific objects postulated by microscopic theories cannot, so the objection runs, be adequately regarded *simply* as conceptual means for organizing and analyzing the objects and events of gross experience.

It must be acknowledged that on this point Dewey is not free from ambiguity. On the one hand, he repeatedly asserts that scien-

tific objects are general *modes* of activity, that they are discursively elaborated *formulations* of connections noted in experience, that they are *correlations* between changes, or that they are *generalizations* of existential conditions founded on a statistical basis. Careful readers of Dewey do not find it overly difficult to see the congruence between these various characterizations. But on the other hand he must cause discomfort even to a sympathetic reader when he writes of swarms of atoms and electrons moving rapidly, or of atoms giving rise to qualities of bitter and sweet. For surely, modes of activity, formulations, correlations, and the rest, cannot intelligibly be said to engage in motions, rapid or otherwise, or to give birth in time to sensory qualities.

It is perhaps doubtful whether these apparent inconsistencies in Dewey's statements can be reconciled; and the task will not even be attempted here. But something does need to be said in defense of his account of scientific objects against the critique which draws its ammunition from the prevalence of microscopic theories in modern physics.

It must be noted, in the first place, that the interpretation of scientific objects as systems of relations between things encountered in ordinary experience and as generalized directives for analyzing empirical materials, is intended *primarily* if not exclusively to be an account of the status and function of scientific objects in the *context of inquiry*. And as has already been indicated, whatever else may be validly said about theories that postulate sub-microscopic particles and processes, this much at least can be asserted with warrant—their role in *inquiry* is that of directives for handling observational materials and of formulations which express systems of relationships between such materials. For unless it were possible to connect such theories with things and events that are open to direct observation, the theories would contribute nothing to the resolution of the specific existential problems which generate them. The "hidden" particles and processes postulated by theories of this type thus serve as intermediary and auxiliary termini in the formulation of a system of relationships whose *ultimate* termini are features of things identifiable in gross experience. Accordingly, the cognitive function in inquiry of the postulated elements in microscopic theories is that of links in a system of conceptual means for treating effectively objects in familiar experience.

In the second place, there is good reason to believe that at least in some cases the postulated elements of microscopic theories are best conceived as complexes of relationships, rather than as concrete individuals comparable with the substantial things of gross experience. This is not the occasion for arguing this point in detail or with reference to specific microscopic theories. However, it is at least worth noting that in the history of mathematics a similar conclusion has been repeatedly reached; for example, instead of construing complex numbers or the "imaginary" points of intersection of curves as a distinctive and self-subsisting type of number or point, it has been found possible to view these postulated "entities" as relational structures between the more familiar integers or "real" points. A *literal* reading of the formulations of pure mathematics may therefore be a naive and misleading one, for such a reading may overlook the operative significance of what is being said. Similarly, a literal reading of the language of modern quantum theory may also be misleading, especially since this theory often uses familiar modes of speech (for example, the expressions "particle" and "momentum") in senses that are explicitly recognized to be analogical and metaphorical. There is indeed an influential if not thoroughly consistent tendency among physicists to-day to dispense with interpretations of even microscopic theories in terms of pictorial models, and to see in the relations expressed by the pictorially neutral mathematical formalism the essential operative content of such theories. An example of this tendency is supplied by a recent commentator, himself a mathematical physicist, on the perplexities generated by the pictorial representation of light as both wave-like and corpuscular: "The picture of what is happening in an optical experiment can be construed in more than one way, and . . . different pictures, although they require different methods of mathematical treatment, yield the same end-result for comparison with observational measures." (Sir Edmund Whittaker, *From Euclid to Eddington*, p. 138.) Accordingly, there is much in the actual practice of modern physics to support the view that the postulated scientific objects of even microscopic theory should be taken to be patterns of relations rather than concrete individuals and processes. The conception of scientific objects as conceptual means for analyzing and comprehending the things encountered in experience in terms of their mutual relations, is thus not without a sound foundation.

Nonetheless, the possibility cannot be excluded, certainly not in a wholesale fashion or on apriori grounds, that the postulated elements of microscopic theories *may* be concrete individuals, and that the detailed empirical evidence *may* warrant the conclusion that these elements are more than patterns of relations. Dewey's concern with natural science has focussed primarily on issues of logic and method, and he has paid relatively little attention to this possibility. His theory of scientific objects as simply conceptual means for the conduct of inquiry, can therefore not unjustly be charged with being an incomplete account of the nature and function of such objects. At the same time, it is worth noting that there is no incompatibility between maintaining that scientific objects function as conceptual tools in inquiry, and holding that in addition they play a role as elements in the executive order of nature. An incompatibility would be generated only if the former function of scientific objects were claimed to be their exclusive one.

Dewey's preoccupation with the instrumental role of scientific objects in inquiry is in part a consequence of his identification of knowledge as the outcome of inquiry into specific, individual existential situations. When knowledge is so identified then, as he himself notes, "The full and eventual reality of knowledge is carried in the individual case, not in general laws isolated from use in giving an individual case its meaning." (*Quest for Certainty*, p. 208.) Indeed, though he recognizes that the word "knowledge" has many meanings, he maintains that in the sense he deliberately assigns to it, it has an especially liberal and humane meaning·

> It signifies events understood, events so discriminately penetrated by thought that mind is literally at home in them. It means comprehension, or inclusive reasonable agreement. What is sometimes termed "applied" science, may then be more truly science than is what is conventionally called pure science. For it is directly concerned with not just instrumentalities, but instrumentalities at work in effecting modifications of existence in behalf of conclusions that are reflectively preferred. Thus conceived the characteristic subject-matter of knowledge consists of fulfilling objects, which as fulfillments are connected with a history to which they give character. Thus conceived, knowledge exists in engineering, medicine, and the social arts more adequately than it does in mathematics, and physics. Thus conceived, history and anthropology are scientific in a sense in which bodies of information that stop short with general formulae are not. (*Experience and Nature*, pp. 161-162.)

Once this standpoint is taken, almost everything Dewey has to say

about scientific objects is a direct consequence from it. But though the sense he selects for the word "knowledge" is not the expression of a personal whim or preference, and though his manner of identifying knowledge is supported by the long history of men's efforts to master their environment, it is also evident that from other standpoints other things may require to be said about scientific objects. For in the enterprise of science the word "knowledge" is frequently employed, as Dewey quite readily acknowledges, to refer not only to the outcome of inquiry into concrete individual affairs, but also to the theoretical vision of pervasive orders in nature which inquiry makes possible. An account of scientific objects which ascribes to them an instrumental role for achieving knowledge in *one* of the several senses of this word, clearly does not constitute a fully adequate contextualist analysis of their function and nature.

6.

There are other philosophies than Dewey's which seek to defend the objective reality of common-sense objects. But unlike most of them, Dewey does not achieve his aim by viewing the whole of nature in terms of distinctions that are known to be relevant only for the human scene. He does not offer a resolution of the standing problems of modern philosophy by clothing all of nature with anthropomorphic traits, or interpreting the course of cosmic events in terms of values that are of paramount concern only to men. The continuity between man and nature which modern theories of knowledge have helped to undermine and which Dewey wishes to establish, does not obliterate fundamental differences between features that clearly characterize only human actions and those which are presumed to be common to all things without exception. Whatever else it may or may not achieve, Dewey's philosophy of science does not offer a view of things that is more obfuscating than the one it wishes to replace. It is a genuine contribution to the clarification of a basic human enterprise.

However, in spite of the brilliant dialectical skill with which Dewey develops his interpretation of science, and notwithstanding the substantial evidence that the history of science provides for it, it cannot be claimed that his views on this subject have won general assent. The difficulties of his literary style are notorious, and have handicapped even first-rate minds in their attempt to penetrate to

the essentials of his philosophy. Moreover, his theory of logic is in effect and by implication a serious intellectual threat to social views whose chief support is tradition and authority; and it would be utopian to suppose that antecedent ideological commitments on the part of his readers have not played a role in their evaluation of his conception of science.

But there are also less external reasons for the hesitations which even those in full sympathy with Dewey's aims and over-all conclusions have experienced with his account of natural science. The great William Harvey is reported to have said of Francis Bacon that he wrote about science like a Lord Chancellor. Of Dewey it can be said with equal justice that he writes about natural science like a philosopher, whose understanding of it, however informed, is derived from second-hand sources. With rare exceptions, the illustrations he supplies for his major theses on the nature of physical science and its method come from every-day inquiries of a fairly elementary kind, or from popularized versions of the achievements of theoretical physics. It is indeed curious that a thinker who has devoted so much effort to clarifying the import of science as has Dewey, should exhibit such a singular unconcern for the detailed articulation of physical theory. His writings often give the impression that however sound his views on the status of scientific objects may be, they have been arrived at by apriori reasoning from general assumptions concerning the nature of knowledge which he takes to be warranted on other grounds. He does not use the language customary in discussions of scientific objects, possibly because he believes this language to be so burdened with traditional associations that in employing it one become hopelessly entangled in the preconceptions of dubious epistemologies. In any event, however, the general absence from his writings of detailed analyses of specific theoretical constructions, as well as his tendency to introduce distinctions that are not fully explained, have contributed to the feeling of inconclusiveness which many of his readers share.

There is a disparaging sense, moreover, in which Dewey has been a lone wolf in the formulation of his ideas on science. His central views are in close agreement with conceptions that have been developed during the past half-century by eminent physicists concerned with the methodology of their discipline. Nevertheless, though he is obviously familiar with many of these analyses, he does not appear

to have been strongly influenced by them, and he cites them only rarely. But what is more to the point, he does not use these specialized and expert studies to the best advantage in his own discussions; and he employs home-grown arguments and formulations even where more convincing ones are readily available. This tendency no doubt contributes to the freshness of what Dewey has to say; but it also serves to isolate him from important and allied contemporary streams in the philosophy of science.

It is not easy to escape the conclusion—a judgment which is perhaps true of all philosophic ventures—that Dewey's discussion of the relation of physics to ordinary experience constitutes a program of work to be done rather than a systematically complete analysis; and Dewey is probably the last man on earth to suppose that nothing further remains to be said on the subject. But it is a program which is accompanied by a refreshingly sane and wise perspective on things, a clarifying conception of the significance of science, and a wealth of brilliant apercus. In stating it, he has succeeded in showing beyond reasonable doubt that the traditional and fatal antithesis between science and ordinary experience is gratuitous, and he has thereby helped to remove some of the intellectual obstacles to the expansion of science and to the consequent enhancement of human life. In stating it, he has also entrusted to those who must succeed him in the endless task of criticism which is philosophy, an objective for whose realization the best energies of thinking men would not be misspent.

7

Dewey's Reconstruction of Logical Theory

I.

SINCE THE TURN of the present century Dewey's thought has been steadily preoccupied with the character and conditions of controlled inquiry; and he has no less insistently claimed that the elimination of many of the contemporary confusions in morals, the sciences of man, and social policy, calls for a reconstruction of logical theory. He himself records that his early interest in logic was stimulated by the intellectual scandal involved in the common separation of science from morals; and he attributes his development of instrumentalism to his conviction that "the construction of a logic, that is, a method of effective inquiry, which would apply without abrupt breach of continuity to the fields designated by both of these words, is at once our needed theoretical solvent and the supply of our greatest practical want."[1]

1. Contemporary American Philosophy (edited by Geo. P. Adams and Wm. P. Montague), Vol. II, p. 23.

Dewey has, however, not only repeatedly called for the reconstruction of the theory of inquiry, but has himself proposed such a reconstruction. Whether as a consequence the confusions to which he has called attention will in the future be less frequent, is problematic. In any case, we still have with us uncriticised preferences masquerading as authoritative values, force parading as reasonableness, dogmatic appeals to the self-evidence of certain goods and norms offered as the essence of wisdom, arbitrary juxtapositions of factual material presented as social science, or the formulation of goals without consideration of the mechanisms required to implement them as significant social planning.

Nevertheless, there has been a change in the intellectual atmosphere since the turn of the century, and only the ignorant would maintain that Dewey has had no influence in effecting it. We are less willing than before to appraise the merits of economic and social proposals without calculating their probable consequences, and without controlling their possible issues by painstaking factual evidence. We are suspicious both of large-scale generalizations in social theory and history which are supported primarily by appeals to alleged necessities of thought, and also of statistical and other factual studies which are not controlled by clearly formulated theoretical assumptions grounded in ascertained facts. We are less easily taken in by the claims of scientific workers that their most recent conclusions are revelations of a final reality, or that the problems of society and morals must be conceived and resolved in terms of the specialized findings of some branch of physics. We are no less sceptical of philosophers who dismiss the specific conclusions of the sciences as not being genuine cases of knowledge and as not being relevant to our understanding of nature; and we do not find plausible their claims to a more profound and more certain theory of the universe than emerges from the detailed studies of the sciences, if it is not supported by the type of investigation which we identify as scientific method. We are not so easily deceived or frightened by statements supported by scientific or philosophical authorities that nature is nothing but a dance of atoms or electrons, that it is nothing but the manifestation of spirit, that it is something intrinsically unknowable, that human experience is nothing but an appearance, or that the nature of things in general places some interdiction or special blessing upon social change. We have become conscious of

how easy it is to transpose distinctions in one context to other contexts in which they have no ascertainable sense, and we have learned to estimate the meaning and validity of propositions in terms of their function and operative rôle in determinate situations. In short, we are conscious of having at our disposal an effective instrument of criticism, a set of logical principles in terms of which claims to knowledge may be approximately adjudicated, and in terms of which programs of action may be controlled.

It is not easy for anyone who has not himself lived through the great intellectual changes of the past half-century to estimate just how much of this instrument of criticism is directly the product of Dewey's thought. Other influences than those coming from his writings have played upon the contemporary scene, and Dewey would be the last one to maintain that he owes nothing to his intellectual predecessors and contemporaries. Moreover, to many a reader brought up in the intellectual atmosphere created at least in part by Dewey, Dewey's books contain much that now seems common-place and matter-of-course, and, like Shakespeare and the Bible, appear to be full of good quotations. Dewey is now part of our common intellectual heritage, though not everything he has taught has found common acceptance. Accordingly, the freshness and historical significance of his approach to logical theory can best be recovered and understood by placing it in the context of the problems which demanded a reconstruction of logic.

Dewey has explained that with the break-up of the medieval intellectual synthesis, due largely to the rise of modern science and its mathematical and experimental techniques, new systems of logic were launched which attempted to formulate the conditions and the methods found successful in achieving credible and warranted beliefs. However, "there soon appeared a division which, while technical in outer appearance, may be said to have had an almost tragic effect upon the intellect of the western world. This was the split between those who appealed exclusively to experience in the form of sense perception as the source of valid beliefs and those who appealed to reason in the form of mathematical concepts as the ultimate authority."[2] But while in the natural sciences this dualism of sense and reason was harmoniously solved in the actual practice and the habits of scientific workers, the resolution of the difficulty

2. *Encyclopedia of the Social Sciences,* Vol. 9, p. 601.

did not receive a coherent theoretical formulation. In consequence, the split had a disastrous import for the still embryonic sciences of man and society, where the method of reason and the method of experience vied with each other for exclusive control and successively dominated research—with the consequence that these disciplines remained barren of warranted conclusions and contributed controversy rather than illumination to the pressing problems of modern society. Accordingly, a reconstructed logical theory is needed. This logic must generalize the acknowledged methods of the natural sciences—methods which in practice harmoniously unite observation, theoretical construction, mathematical reasoning, and experimental verification—and must exhibit the traditional separation of sense and reason as theoretically unwarranted. The reconstructed logic will thus insist that "ideas and principles must be employed to deal overtly and actively with 'facts' if, on one side, the facts are to be significant and if, on the other, ideas and theories are to receive test and verification. Experimental logic would resolve the controversy, now four centuries old, between reason and sense experience, by making both concepts and facts elements in and instruments of intelligently controlled action."[3]

If this has been the goal of a reconstructed logic—a reasonable goal, it will seem to many—why has it not already been achieved and what obstacles have stood in its way? Other men in other generations have been preoccupied with problems of intellectual method, and it may seem strange that it should have remained for our period to attempt the theoretical synthesis of these contending claimants to intellectual authority. The answer in this case as in many others of like kind is historical, and points to the accumulation of specific scientific findings in our own age which have been absent in previous ones.

One hundred fifty years ago a great philosopher could maintain that since Greek antiquity logic has not had to retrace a single step nor been able to make any further advance, so that, accordingly, logic was complete and perfect. In the light of the intense activity during the past three generations in correcting and expanding the content of logical theory beyond recognition by its founders, this estimate is a curiosity; it supplies more hints concerning the philo-

3. Encyclopedia of the Social Sciences, Vol. 9, p. 603.

sophical outlook of the men of the eighteenth century than about the subject concerning which it was made. Indeed, there is no reason to suppose that, granted a favorable cultural milieu, such an intense activity in logical researches will not continue indefinitely into the future. Why then did a man of unusually shrewd intelligence and learning, and why do some of our own contemporaries with perhaps no less intelligence, commit themselves to such an appraisal?

To many reflective minds of the eighteenth century the Newtonian conception of nature seemed to be the paradigm upon which every rational science must be modeled, and to whom the Newtonian theory of science constituted the set of necessary principles for acquiring, ordering, and interpreting all knowledge. The geometry of Euclid and the mechanics of Newton were the only familiar systems of deductive mathematics, the properties of things capable of formulation in the then existing mathematics were taken to be the sole objective properties of things, and the limitations and defects of known mathematical techniques were assumed to mark off the inherent boundaries of human reason. It was, accordingly, a period much concerned with discovering the logical foundations for allegedly indubitable truths concerning the physical world and our powers of apprehending them, and with constructing an intellectual foundation for moral and esthetic judgments so as to render them congruous with the logical and physical assumptions of the natural sciences. Lack of knowledge of alternative comprehensive mathematical and physical systems was construed as evidence of their impossibility, and the contemporary achievements of the sciences were riveted into the final structure of nature and reason. Biology, moreover, was still largely a classificatory discipline. The physiological and animal basis of human thought was only dimly understood, and human reason, occurring in a world conceived in terms of the specific categories of the dominant physics, was set off sharply from the humbler practices of the common arts. Given the conceptions which dominated the sciences of the day, given the sense of finality and certainty with respect to the conclusions of the physics of the day, it is not strange that logical theory itself should have been regarded as perfect and as immune to modification. And to the extent that contemporary thinkers are still dominated by philosophies devised as intellectual bases for sciences so conceived,

it is understandable why they also should formulate similar opinions as to the proper content of logical theory.

Within seventy-five years, however, the intellectual scene was already shifting, and the renaissance of logical research was begun. Leading the van of the events which stimulated the growth of newer conceptions of knowledge and nature were the achievements of the mathematicians. They discovered new systems of geometry which were consistent alternatives to the ancient discipline, and thus challenged the authority of theories of knowledge which granted exclusive validity to the system of Euclid in particular and assumed the possibility of apodictic knowledge of existential matters in general. They invented and systematized new forms of algebra and new techniques of analysis, which gradually undermined dogmatic appeals to self-evidence as criteria of factual as well as mathematical truth; and they thus called attention to the fundamental rôle which symbolic operations play in the development of deductive systems and in the attainment of knowledge of matters of fact. No less significant was the growing awareness of the limitations of traditional canons of formal validity. Mathematicians and mathematically trained students of formal logic soon developed algebraic algorithms which were startlingly effective constructive and critical devices for handling complicated types of deductions such as occur in mathematics—deductions which were not easily treated by traditional methods; and they soon established the fact that the restriction of formal logic to the theory of the syllogism, the conception of formal mathematics as the science of simple quantities, and the isolation and theoretical separation of the techniques of formal logic from those of mathematics were unwarranted prejudgments. Mathematicians and formal logicians thus constructed impressive intellectual tools that served a double end: on the one hand, they became means for the development of comprehensive sciences of nature which were to challenge the claims made for existing theories as the only conceivable ones; and on the other hand, they supplied crucial evidence for testing the adequacy of conceptions of knowledge and inquiry.

These developments in the formal sciences went hand in hand with those in the physical disciplines. It was discovered before long that the specific categories of Newtonian mechanics had only a limited applicability. For the task of bringing into systematic order vast ranges of phenomena (those studied, for example, in optics,

electricity, or thermodynamics), required the introduction of characteristic modes of analysis which differed in important ways from those employed by Newton. The theory of probability, heretofore used almost exclusively only in connection with games of chance and errors of observation, now came to be employed as an integral part of physical theories themselves, and was extended with some success to the study of social statistics. In consequence, the older conceptions and formulations of the ideals and logical structure of scientific knowledge were given a critical overhauling. It was seen that the assumptions of classical physics were not universal necessities of nature but were only somewhat parochial principles of analysis suitable for handling a limited type of material; and it was shown that at least in some cases what were previously assumed to be inherent necessities of reason were simply the analytic consequences of adopting one of several alternative techniques of measurement. These innovations in the natural sciences, as well as the growing abstractness and non-descriptive character of physical concepts, led to an intense concern with the relation of theory to every-day experience; and outstanding workers in the sciences themselves contributed illuminating studies on the links connecting theoretical formulations in the sciences with the familiar materials and activities of every-day observation. It became evident, for example, that though physics was abstract, physical theories were not abstracted from sensory qualities; and it also became apparent that if the meaning of theories was to be understood, that understanding was not achieved by regarding them as photographic reports of some hidden reality. In general, therefore, theoretical formulations were recognized as intellectual constructions devised for specific ends, so that their meaning and validity must be determined within the specific contexts in which they were introduced—in terms of the operations (observational, experimental, and mathematical) relevant to their normal functioning. By increasing our knowledge and by suggesting alternative possibilities of formulation and analysis, these developments in modern science and mathematics have had the effect of travel in broadening the mind and freeing it from local and traditional prejudices. Accordingly, any conception of knowledge and inquiry which cannot square itself with these developments—any logic which demands, for example, indubitable first-principles and incorrigible conclusions of inquiry—cannot consti-

tute an adequate novum organum for guiding and criticizing scientific research.

The perhaps inevitable scepticism and agnosticism which might have become pervasive as a consequence of these intellectual changes, was counteracted by the impact of evolutionary biology upon conceptions of logic and knowledge. Thinkers who took seriously the emphasis of modern biology upon the genesis of human functions from an animal basis and upon their rôle in adapting the organism and its environment to each other, found it impossible to hold that human reasoning powers did not also have a history, or that the rational faculties do not also have an adaptive function. They found it implausible to believe that the latest conclusions of the sciences were the final ones, that the human organism was just the passive recipient of impulses from the physical world, or that scientific research and its outcome were not themselves at least partly instrumental in the process of biological and social adjustment. But these supposals, instead of leading to scepticism as to the possibility of knowledge, can readily engender a contrary effect. For if thought is a biological process, the outcome of thought is adequate to its object to the extent that it permits a more satisfactory adaptation of organism and environment. If the present instrumentalities for acquiring knowledge did not emerge full-grown with the beginning of the race, but are the products of historical trials and errors, their validity and adequacy must be construed and tested in terms of the success with which they make possible an adjustment between organic needs and physical environment. And if knowledge, conceived in the fashion of an infallible grasp of final truths without the mediation of overt organic activity, is not something which modern science supplies, then knowledge must be conceived in such a way that it is just the sort of thing scientific methods do achieve. Thus one important consequence of modern biology upon conceptions of logic has been the rejection of an antecedent or apriori definition of knowledge, and the substitution for it of something which can be identified as the product of an identifiable procedure. More generally, modern evolutionary biology supplied fresh clues for resolving the dualisms between mind and nature, or reason and sense. For according to it, mind is to be construed not as a mysterious power capable of grasping with finality the structure of a world alien to it, but as a specific mode of behavior of organic beings in various

contexts. In consequence, human thought appears as continuous with the physical and biological activity of bodies, and the reconstructive and inventive powers of reason are grounded in the complicated mechanisms of the human organism. It thus became possible to regard the events and qualities occurring in the human scene as much as the products of natural processes, and as genuine clues to the structures and mechanisms of nature, as are the objects discovered or postulated in the science of physics.

This rough inventory of the fresh materials thrown up by almost a century of research supplies the background for the reconstruction of logical theory which Dewey has offered. Some of this material began to influence his thought only at later periods of his development. Accordingly, his attempts to work into one coherent theoretical formulation the instrumentalities of modern science exhibit shifting stresses—shifts which reflect the impact of new and frequently unconnected contributions to methods of inquiry upon a mind sensitive to the complexity of his subject. He once confessed that he appeared to himself as "unstable, chameleon-like, yielding one after another to many diverse and even incompatible influences; struggling to assimilate something from each and yet striving to carry it forward in a way that is logically consistent with what has been learned from its predecessors."[4] The shifts in Dewey's thought, examined in terms of the specific influences which played upon him, would be the study not only of an individual thinker but of a period.

2.

What then, granting this background and omitting technical details, are the outstanding features of Dewey's conception of logic?

1) Perhaps the most characteristic trait of Dewey's reconstructed logic is the setting or context he supplies for the consideration of logical distinctions and principles. He takes for granted that reflective thinking is an activity which can be identified as genuinely as can the interplay of physical and chemical forces; and he assumes as a hypothesis that it is an activity which occurs only when some felt difficulty (theoretical or practical) in a situation calls it forth as an instrument for its resolution. All of Dewey's specific analyses are thus tacitly based upon an appraisal of the history of men's efforts to achieve satisfactory solutions of their problems, and upon

4. *Contemporary American Philosophy,* Vol. II, p. 22.

a comparative study and evaluation of different generalized methods men have used in removing obstacles to smoothly flowing activities. Methods of inquiry are accordingly not private, individual affairs. Judgment of their soundness requires the study of human history in order to determine which of the methods actually employed have, in the long run, been satisfactory in the resolution of difficulties. Dewey's conception of logical theory is an immediate corollary from this initial identification of its subject-matter: "if thinking is the way in which deliberate reorganization of experience is secured, then logic is such a clarified and systematized formulation of the procedures of thinking as will enable the desired reconstruction to go on more economically and efficiently."[5]

In consequence, to those who identify logical theory with formal logic, Dewey's claim to the title of logician has never seemed appropriate, and he has been adjudged by many as simply a descriptive anthropologist or psychologist. It is not profitable to quarrel about labels, but it is worth remarking that if, for example, Dewey's writings are classified as belonging to anthropology, the content of this discipline must be enlarged beyond its usual bounds. For though the situations in which, according to him, logical distinctions are relevant are situations of inquiry and therefore of human activity, his attention is directed upon what the operations of logic contribute to the specific goal of inquiry (that is, to the controlled resolution of problems). His concerns are therefore evaluative rather than descriptive. If Dewey's logical theory runs the danger of being confused with psychology, he gladly takes the risk because he is concerned with specifying in just what contexts the familiar logical operations (e.g., definition, classification, deduction, etc.) are valuable and valid. It is essential to specify these contexts in order to prevent an unwarranted transference of those values and norms to other contexts. For indication of the situations in which logical devices are actually employed closes the door to interpretations of the identifiable functions of logical principles in ways not relevant to, or not compatible with, their identifiable operative rôles in inquiry.

Dewey's insistence upon construing the meaning of logical operations in terms of their specific functions in the context of inquiry is one of his most illuminating insights, though at the same time it

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5. *Reconstruction in Philosophy*, p. 134.

has been the source of most of the criticisms that have been levelled at him. Consider, for example, the distinct but related rôles he assigns to sensory observation and reasoning—thereby harmoniously resolving the dualism which has been one motivation for his reconstruction of logic. The occurrence of qualitative perceptions, reveries, speculations and fancies in contexts other than that of inquiry is controlled by habit, chance circumstance, or native propensity. But within a reflective context the situation is otherwise. The nature of the problem in situations involving felt difficulties does not usually spring into full view immediately, and must be sought for and identified. Observation then becomes controlled by suggestions or hypotheses, and sensory qualities are not received passively but are deliberately selected and isolated from a larger qualitative context. The identification of the problem is thus achieved neither by subcutaneous thought nor by random activity, but by overt activity controlled by ideas. Moreover, when the problem has been located, attempted solutions of it involve further controlled observation, instituted by hypotheses for the sake of obtaining factual evidence for them. Accordingly, sense and reason are cooperatively engaged throughout controlled inquiry. Perceptual material, gathered in the light of entertained suggestions, serves as means for locating the difficulty and supplies critical evidence for proposed solutions of it; ideas or hypotheses, without being simply abstracted from materials of sense and without being self-authenticating, serve as plans of research, as ways of obtaining and ordering sensory material, and as suggestions for so rearranging existential material that the initial difficulty may disappear. The dualism of sense and reason is thus not an insoluble philosophical problem in inquiry.

Consider again Dewey's discussion of the role of those formal principles which in logic are usually characterized as necessary truths. They are shown by him to be capable of playing two rôles: that of procedural principles, which state what conditions must be instituted in order to make unambiguous the reference of terms; and that of transformative rules for, or intermediary links in, operations upon and with symbols. It is no doubt possible to interpret the necessary laws of logic as formulations of fundamental invariants of all nature, and the like. The pertinent question, however, is whether such interpretations are at all relevant for ascertaining the significance of such principles in the context of inquiry. It is

Dewey's singular merit to have shown that in *this* context the manifest function of these propositions constitutes their one identifiable meaning. The history of thought reveals a progressive diminution in the number of propositions which are assumed to be capable of demonstration without appeal to experiment and observation, but which nevertheless are claimed to formulate factual relations between properties of things. Propositions belonging to the natural and social sciences are necessary and irrefutable by experience, it is now clear, only if, in a broad but identifiable sense, they are definitory; otherwise, they are contingent truths, corrigible by further inquiry. Only in logic and in some branches of mathematics does the claim continue that propositions in these disciplines represent apriori knowledge of the structure of things. The analysis Dewey has made of necessary propositions may not finally dispose of the issue to the satisfaction of everyone. But his analysis does show at any rate that when the propositions of logic are construed in terms of their functions in inquiry, the claim that those propositions formulate apriori truths about existence is entirely gratuitous.

Or consider, as a final example, Dewey's resolution of the oft-discussed question as to the "criteria" of truth. It is patently desirable and important to have a method for identifying genuine cases of knowledge, and for distinguishing between valid and spurious cognitive claims. To be sure, it may not be possible to persuade a wholesale sceptic or dogmatist to recognize such a distinction. The distinction nevertheless is tacitly recognized when, for example, we accept as reliable the contents of an Ephemeris, but not the horoscope of an astrologer. What, then, are the earmarks of knowledge that is authentic, and in what way does such knowledge differ from ungrounded belief? Philosophers have sought to answer such questions by proposing various "theories" of truth, each of which ostensibly supplies not only an analysis of what is the meaning of "truth" but also a criterion for distinguishing the true from the false. These alternate "theories" are, however, in sharp disagreement with one another; and what is more to the point, their proposed criteria of truth turn out to be quite irrelevant to the actual problems of inquiry. But whatever be the philosophical theory of truth that one may adopt, the validity of truth-claims is identified on the basis of whether or not the claims are products of competent inquiries. At any rate, this is Dewey's view. He identifies

authentic knowledge (which he calls "warranted assertibility") as that which is achieved by an inquiry whose features conform to a determinate logical pattern—a pattern exhibited in those inquiries in the history of man which have shown themselves effective in obtaining solutions for the problems generating the investigations.

However, anything identified as knowledge on the basis of being the product of such inquiries will embody the character of the logic employed. We must not expect that an assertion warranted by scientific method is above revision, for the method of science is not infallible. That method is, however, progressively self-corrective, and provides for the revision of its products in the light of further controlled inquiry. The degree of confidence we are entitled to have in an assertion is therefore a function of the thoroughness with which we have tested it with the best methods available. We must, accordingly, rely on the integrity of the logical method we employ to secure dependable conclusions and to identify and distinguish the true from the false. There is no way of discovering what things are like before we have inquired into them, and there is no way of ascertaining the adequacy of a logical method before we have exercised it to reveal its powers and limitations. All conceivable methods of conducting inquiry have an equal apriori claim on our attention; but historical experience has taught us that not all methods are equally effective. If we do not wish to be constantly betrayed by claims to knowledge, we must institute modes of inquiry in accordance with certain logical requirements. The general problem of truth, on Dewey's view, is thus the problem of perfecting methods of inquiry which, in the long run, exhibit themselves as competent to do the job for which they are invented.

2) It will now be apparent that on Dewey's conception of logic, logical principles are formulations of conditions which must be instituted if an inquiry is to satisfy the conditions of being a controlled one. Accordingly, logical forms are understood by him to be identifiable traits which things and events acquire only upon entering inquiry and which things do not possess antecedently. This is a view which many thinkers have found too difficult to accept; and indeed it is a truistically false view if "logical form" is taken to designate certain structures of "facts" or statements, whose existence does not depend upon these elements entering the context of inquiry. What then is Dewey's meaning? According to

him, things and qualities acquire *functions* in inquiry which they did not have antecedently to it, and the specifically distinct *ways* in which things function in this context is just what he understands by logical form. For example, perceptual material in its sheer existential status does not have the logical form of being evidence, nor does the occurrence of an idea in revery have the status of a hypothesis—any more than a bear is always a target—except on the occasions when these items enter appropriate contexts involving specific types of activity.

This way of looking upon the matter represents a self-conscious attempt to steer a median course between an empiricism for which logical traits are psychological, in the sense of being mental, and a realism for which logic sets apriori limits upon the characters of existence. Dewey's conception of logic thus offers the alternative hypothesis, according to which logical forms are not mentalistic, and yet are traits of things (that is, modes of behavior) only when things are caught up in reflective inquiry and are subjected to the conditions required for inference. This conception supports the analysis of logical propositions as instrumental in reflective inquiry, because they are taken to specify the means for securing the conditions (that is, for installing the requisite logical forms) which will carry inquiry to a successful termination.

It must be noted, however, that while logical principles are not formulations of relations between things independently of their occurrence in inquiry, they are not arbitrary rules. On Dewey's view, what the conditions are for satisfactory inquiry is a discovery about them, and is as much a discovery as that the human body needs food to survive or that sound requires a material medium for its transmission. In general, therefore, logical principles are just one special class of formulations which state the conditions or means for the attainment of ends or consequences. If the notion of logical principles as instruments or tools is taken seriously, their adequacy must evidently be contingent upon the character of the materials upon which they are employed and upon the objectives they are to attain; and there is no way of discovering whether they are indeed adequate until we have made the attempt to use them. Dewey is thus in agreement with all those who have maintained (sometimes in criticism of what they take to be his position) that in some sense principles of inquiry are grounded in the nature of things. They

are, for him, grounded in the requirements of controlled inquiries, which are manifestly matters of existence. But one must add that just as wood-pulp is not paper, and is potentially paper only in relation to determinate chemical transformations, so a segment of existence is not intrinsically evidence or datum, and it possesses such logical functions only in relation to the equally determinate operations of controlled inquiry.

Dewey's reconstruction of logic thus involves what has been called a genetic approach, so that, consequently, teleological categories are prominent in his theory. He has evaluated logical methods in terms of a natural history of knowledge, and he has been concerned with their efficacy in transforming ignorance and error into warrantable and reliable science. He has accordingly tried to understand the refined instruments of intellectual analysis as devices having their origin in the activities of biological organisms coping with their environment, and as factors contributing to the achievement of specific socio-biological ends. Inquiry is thus a genetic process, having natural beginnings and endings, and the continuum of inquiries supplies the materials for appraising the meaning and the worth of the mechanisms involved in achieving those ends. On Dewey's view, therefore, the "justification" of logical methods must always be given on historical grounds, and not on grounds which derive their authority from anterior criteria of validity. And in consonance with his identification of logical operations as factors in a process, his analysis of them is in terms of the general functions they perform in that process. This, perhaps, is the heart of his logical theory.

Dewey's genetic approach has, however, brought him the rereproach that his theory is based on the genetic fallacy. It is certainly a confusion to mistake questions of temporal origin or temporal order with questions of adequacy and validity; and, regrettably, Dewey has sometimes argued, or at least given the appearance of so arguing, that the validity of previous logical systems may be determined on the basis of the character of the historical period during which they originated. But the use of the genetic method does not necessarily involve the commission of the genetic fallacy. It is a fallacy to declare that the theory of the syllogism is faulty because it was first formulated at a time when detailed knowledge of mathematics and physics was inferior to our own;

but it is not a fallacy to insist that the adequacy of this theory as an instrument of inquiry and criticism must be estimated in terms of how it has hindered or advanced the pursuit of knowledge throughout its history. Nor is it a fallacy to maintain, on the ground that knowledge is the product of inquiry, that the logical worth of a conclusion must be evaluated in terms of the methods employed to reach and test it; for a statement considered apart from the evidence for it is not a conclusion at all, and cannot be assigned any cognitive status.

In general, therefore, the validity of a belief is not determined by its origin. But if a belief, taken as the conclusion of controlled inquiry, involves a prior reconstruction of experience, and if a belief, to be rightly understood, requires an understanding of the methods of testing and criticising it, then its meaning as well as its validity can not be rendered without reference to the procedures of which it is a product. We mislead ourselves if we suppose that we understand what a physicist tells us about the latent energies of atomic nuclei or what a biologist tells about the structure of chromosomes, if we neglect the contexts in which such statements have relevance and if we are ignorant of the operations in terms of which nuclei and chromosomes are defined and identified. For it is not knowledge but images that we then possess.

3) A logical theory is not complete if it does not provide some guide to the understanding and appraisal of the refined conclusions of modern theoretical science. The logical issues involved are relevant for the consideration of scientific theories at almost any stage of scientific development; but a guide is particularly important at a time when new developments in sub-atomic physics and in cosmology have given rise to amazingly contradictory philosophic interpretations of them. For example, some writers appraise the latest theories of science as disclosures of an ultimate reality, incompatible with the reality of the familiar human scene; others construe them as simply convenient fictions with no firm standing in the order of nature; and still others offer intricate variations upon these themes.

Dewey's approach, however, challenges the relevance of such interpretations for the understanding of scientific theories, by simply considering the function of theories in the specific contexts in which they are cognitively employed. He finds no difficulty in

showing, for example, that neither in pure research nor in the application of theoretical physics to problems of technology does the issue ever arise whether physics has located an ultimate reality or not. He has no more difficulty in concluding that to ask whether a theory is fictional or not assumes a prior conception of reality—a conception formulated antecedent to the findings of specific inquiries, and which therefore has no cognitive worth.

But his fundamental criticism of these interpretations is that they misconceive the function of theories and fail to study them in their identifiable roles in research and application. These interpretations are predicated on the assumption that it is the business of a theory to offer a description or photographic picture of some segment of nature. But a contextual analysis which pays attention to a theory's function in inquiry shows, that on the contrary, the business of a theory is to organize inquiries, to make possible extensive correlations of measurable properties of things, and to suggest ways of introducing experimental action so as to resolve the specific problems of inquiry. The electrons postulated by a theory are defined exclusively by that theory, and are defined in such a way that they have none of the familiar qualities of objects in daily experience; electrons are thus a set of complicated relations between measurable traits of things, and in inquiry at any rate they can be assigned no reality prior or superior to that of common objects of experience. As Dewey points out,

> That heat is a mode of motion does not signify that heat and cold as qualitatively experienced are "unreal," but that the qualitative experience can be treated as an event measured in terms of units of velocity of movement, involving units of positions and time, so that it can be connected with other events or changes similarly formulated. . . . The resolution of objects and nature as a whole into facts stated exclusively in terms of quantities which may be handled in calculation, such as saying that red *is* such a number of changes while green is another, seems strange and puzzling only when we fail to appreciate what it signifies. In reality, it is a declaration that this is the effective way to *think* things; the effective mode in which to frame ideas of them, to formulate their meanings. . . . Since these correlations are what physical inquiry *does* know, it is fair to conclude that they are what it intends or means to know.[6]

The elimination of qualities from the theoretical formulations of physics does not therefore eliminate them from existence; and

6. *The Quest for Certainty*, pp. 129-134.

while science renders more perfect the cognitive experiencing of things, knowledge is not the exclusive mode of experiencing. Accordingly, even if it were possible—which by definition it is not—to have a qualitative experience of electrons, we would not thereby know them any better or more profoundly than we do at present.

The bearing of his conception of logic upon the interpretation of science is summed by Dewey himself too clearly to require periphrasis.

> There is something both ridiculous and disconcerting in the way in which men have let themselves be imposed upon, so as to infer that scientific ways of thinking of objects give the inner reality of things, and that they put a mark of spuriousness upon all other ways of thinking of them, and of perceiving and enjoying them. It is ludicrous because these scientific conceptions, like other instruments, are hand-made by man in pursuit of realization of a certain interest—that of the maximum of convertibility of every object of thought into any and every other. It is a wonderful ideal; the ingenuity which man has shown in devising means of realizing the interest is even more marvelous. But these ways of thinking are no more rivals of or substitutes for objects as directly perceived and enjoyed than the power-loom, which is a more effective instrument for weaving cloth than was the old hand-loom, is a substitute and rival for cloth. The man who is disappointed and tragic because he cannot wear a loom is in reality no more ridiculous than are the persons who feel troubled because the objects of scientific conceptions of natural things have not the same uses and values as the things of direct experience.[7]

3.

These, in outline, are some of the central themes in Dewey's logical theory. It is an impressive performance, whatever be the final judgment passed on the validity of the plan or the details of his reconstruction. But it is only fair to say that Dewey has not been the first to venture a unified picture of controlled inquiry. The nineteenth century—to restrict mention to the period during which Dewey came to intellectual maturity—witnessed several attempts to formulate comprehensive accounts of the principles of evidence and of the road which the mind travels in order to achieve reliable beliefs; and Dewey has not hesitated to acknowledge his profound indebtedness to most of them. But for various reasons they suffer from limitations which make them inadequate as accounts of the subject with which they presumably are concerned—either because

7. *Ibid.*, p. 136.

they are based on a sensationalistic, pre-Darwinian psychology; or because they show insufficient understanding of the role of mathematical and experimental techniques; or because the full significance for logic of scientific advances was not apparent at the time they were written, or because, finally, since they were undertaken with a frequently avowed philosophical *arrière-pensée*, they failed to appraise and integrate logical principles in terms of meanings assigned to them on the basis of their identifiable functions in recognizable contexts of inquiry. But in any case, something has been learned since then about the subject-matter of logical theory; and every age must rewrite its logic as it does its history.

There is, indeed, already a sense in which Dewey's own work is outmoded. For some of his analyses are directed upon issues which dominated thinkers of the preceding century—problems which, though intelligible enough in terms of the scientific, religious, and social conceptions then current, have lost much of their meaning and relevance for the contemporary reader. It is doubtless important to realize that man is an animal and that his rational powers are extensions of more rudimentary biological functions. But the modern reader will be willing to take that for granted, and he will suspect the generalized reconstructions of this genesis which are not based upon specific experimental findings, and which, even if coherent and cogent, leave him not one whit wiser as to the detailed mechanisms involved. It is illuminating to be told that experience is not constructed out of atomic sensations, and that the inquiring mind is not simply the passive recipient of external stimuli. But many contemporaries have learned that lesson, and they are eager for new ones. They are impressed as well as puzzled by the complicated theories of modern physical science as well as by the difficulties of social science and politics; and they are eager to master a technique which could unravel for them the detailed logical structure of assumptions and procedures upon which these theories rest, and to see the method of intelligence at work in concrete applications. To such readers, at any rate, Dewey does not supply all they are looking for in a logical theory, though he does supply them with an integral vision of the work to be done and of the conditions under which it is to be carried on. The Dewey they find inspiring and rewarding is the Dewey who has insisted, by precept and example, that a contextual (or functional, or operational) analysis

of concepts and principles is the only illuminating and adequate one. On this central issue Dewey has set for them a standard toward which to strive, not only in various details of his specific logical analyses, but in a half a dozen other fields as well: in his clarifying discussions of the reflex-arc concept, of corporate personality, of the public, of the relation between the fine and useful arts, of means and ends, and of freedom and democracy, to cite only a few examples.

It is also only just to add that Dewey has contributed next to nothing to the important recent literature concerned with the conditions of formal validity or with the techniques for developing the implications, the inter-relations, and the consistency of formal systems. Indeed, a strikingly large proportion of his logical writings consists of sharp polemics against those whom he calls "formalists." Though he is often just in his remarks upon the less cautious expressions of the achievements of modern symbolic logic (and especially upon the dubious epistemological use and foundation that Russell has given them), it is frequently difficult to resist the conviction that he has underestimated the significance for his own conception of logical theory of these achievements, or that he has been unwittingly flogging a dead horse. He has himself stated that his own development "has been controlled largely by a struggle between a native inclination toward the schematic and formally logical, and those incidents of personal experience that compelled me to take account of actual material."[8] However this may be, it is a matter for genuine regret that such a considerable part of his logical reconstruction has been carried on without the aid of the rigorous formal techniques now in existence, or without a more circumspect and precise formulation of the distinctions required by his theory which those techniques might have enabled him to give.

But although much of Dewey's writing is controversial—directed against almost all historical and contemporary schools of thought—his use of the polemic illustrates that socialized, cooperative method of science for which he is a spokesman and a pleader. For he has been willing to learn from the subject-matter under discussion as well as from the reflections of others; and his polemic, introduced in the midst of first-hand reports on phases of scientific method, aims to bring into focus just what the issues are for which he is

8. Contemporary American Philosophy, Vol. II, p. 16.

contending and just what are the limitations of alternative approaches as he understands them. In any case, there are few writings in philosophy which represent as brilliant an intellectual performance as the final chapter of his *Logic*, in which he exhibits different types of logical theories as emphasizing some essential phase of inquiry to the exclusion of other phases—with the consequence that, while judged to be inadequate as a coherent account of its subject-matter, the reasons for the plausibility of each theory are accounted for. Reference to the subject-matter of logical theory must obviously be the final basis for evaluating the adequacy of Dewey's own theory. But such an evaluation can be made, as the evaluations of theories in the natural and social sciences are made, only by considering theories alternative to his, and by comparing the relative success with which the available theories have been able to incorporate systematically the rich subject-matter of logic.

This is not the appropriate occasion for a detailed examination of Dewey's reconstruction of logical theory. But it is proper to point out that to some of his readers many of his central technical distinctions are extremely vague if not ambiguous, so that it is frequently impossible to discover their precise import. He has attempted a generalized account of the pattern and operations of inquiry; and he has unquestionably been successful in formulating the common structure of operations that are identifiably similar, despite their occurrence in specifically different investigations. Nevertheless, his general formulas sometimes cover structurally distinct operations, which can be identified as similar only on the basis of hopelessly vague analogies. A theory which is so general that it takes no note of commonly recognized distinctions is always in danger of lapsing into a tautology. Has not Dewey only himself to blame, for example, for the frequent charge of anti-intellectualism and crude utilitarianism levelled against him because of his claim that every inquiry must have "practical" consequences and must employ "practical" means of control? For he has obscured some essential differences by using the term "practice" so broadly that it comprehends not only the operations of industry and the arts, but also the purely symbolic operations such as occur in mathematics. How much ink and misunderstanding would have been saved if Dewey had stated unambiguously what precisely he understood by "object of knowledge," and what, for example, he took to be the status with respect

to knowledge of objects and events which can not be physically transformed by the operations of inquiry. Many of Dewey's contentions turn out to be truistic and not worth fighting about, once they are formulated with care. But words having a familiar meaning cannot be given an unusual one without risk of confusing their readers, unless precise rules are supplied for their application.

Dewey has expressed great hopes for the development of the social sciences if antiquated logical conceptions are replaced by those conforming to the requirements of modern scientific method. However, not all of his readers can share his youthful optimism on this point, since to many of them an adequate theory of inquiry is neither a sufficient nor a necessary condition for the advancement of a science. Dewey has himself pointed out the remarkable achievements of the founders of modern physical science, in spite of their inadequate conception of the logic of their procedure. The intent of this comment is certainly not that of objecting to Dewey's aim to extend scientific attitudes and critical habits of mind into domains usually dominated by ignorance and dogmatism. The question that is being raised is to ask what we may reasonably expect from a logical theory.

Dewey has sometimes conceived his logical theory as a logic of discovery, as distinct from a logic of exposition or of the transmission of already acquired knowledge. There can be no question that he has contributed something other than the latter type of logical theory. On the other hand, the phrase "logic of discovery" has too many misleading overtones of meaning to allow it to serve as a satisfactory characterization of what he has achieved. Of one thing, however, there is no doubt—his logical theory does offer us illuminating principles of criticism and evaluation, it does provide us with intellectual tools with the help of which choices and decisions can be made more rational by making them less arbitrary, and it does impress upon us the moral significance of being intelligent. Diderot at the court of Catherine the Great is reputed to have become disconcerted when the mathematician Euler announced to him: "*a* plus *b* raised to the *n*th power is equal to *x*; therefore God exists." Like Diderot, we may not always be in the position to locate the flaw in the argument, and may become disconcerted; but like him, we may decide it is the greater part of wisdom to refuse to be fooled. Dewey has helped the present generation not to be

readily fooled, and to distinguish genuine knowledge from pseudo-science. And in doing that he has helped us to become intelligent and to cast off the bonds of slavery to superstition and blind habits of mind. His own life and work exemplify the truth of his dictum: "All intelligent thinking means an increment of freedom of action —an emancipation from chance and fatality."

8

Reflections on Some Logical and Metaphysical Themes in Dewey's Philosophy

1. SOME LEADING PRINCIPLES OF PROFESSOR DEWEY'S LOGICAL THEORY

THE CONCEPTION OF LOGIC which Professor Dewey has been proposing since the turn of the century is by now so familiar that it would be pointless to restate it in detail in this review of his latest formulation of it.[1] But no careful reader of this treatise, however faithfully he may have followed Professor Dewey's writings, can fail to be stirred by the energy with which it recaptures the splendid ideal of logic as an organon for the effective conduct of inquiry; nor can he help being impressed by the bold power with which it re-interprets, on the basis of the dominant idea of inquiry as a mediatory process with logical principles as its tools, the themes of traditional logic and epistemology and the diversely oriented analyses of contemporary thinkers. Professor Dewey's previous contributions to logical theory, scattered through a score of books, now

1. *Logic: The Theory of Inquiry*. John Dewey. New York: Henry Holt and Company, 1938. viii + 546 pp. $3.00.

fall into a more definite pattern than before, with the consequence that the grounds of his dissent from, as well as of his dependence upon, traditional and contemporary schools are made more evident.

On the other hand, this volume also makes clear that Professor Dewey has not ceased to look upon his logical theory as a hypothesis, the detailed confirmation of which would have to be supplied by others in the future. His attention remains focussed primarily on the larger context within which logical principles operate, and in which they obtain a significance as something more than elements in a self-contained mathematical system; and even in this most complete formulation of his theory, the various functions which the principles of formal logic have as tools of inquiry are sketched with only enough detail to supply suggestive programs for further exploration. In consequence, the book offers a general underlying conception of logic with which many students will enthusiastically concur, while at the same time leaving them puzzled at or at issue with many of its special analyses. Professor Dewey explicitly recognizes the programmatic character of his reconstruction of logical theory, and like Peirce whom he admires he regards claims to finality as blocking the road to inquiry; accordingly, his inquiry into inquiry must be taken to mark only the beginning of a large project of continued research.

Professor Dewey is a naturalist in philosophy, and he writes of logical forms and principles as would a naturalist in the familiar biological sense of the word. His treatise is a study of the morphology of inquiry, and it aims to exhibit, as a biologist might in connection with various forms and organs of life, the conditions under which logical forms occur and are developed, their specific functions, their dependence upon and their transformation of their environment, and their own interrelations on the basis of their contributions to the achievement of their objectives.

But Professor Dewey is not just a classificatory naturalist. His examination of logical principles is carried through in terms of an inclusive theory, according to which subject-matter acquires logical traits on being subjected to the differential physical transformations of inquiry, in consequence, logical principles are taken to formulate the empirically ascertained conditions which must be instituted if inquiry is to have a successful termination. This cardinal thesis has the force of a paradox, only as long as the methods and conclu-

sions of inquiry are isolated from the contexts in which they operate and emerge by being examined solely on the basis of their finished formulations. The thesis is further clarified if it is noted that in Professor Dewey's usage the phrase "logical form" stands for the *way* in which selected features of a situation in which inquiry occurs *function* in it, while the phrase "logical principles" denotes a *mode of operation*—and not, as is sometimes the case among students of formal validity, for quasi-grammatical forms and rules. Just as plants and animals are studied to the best advantage only within their natural environment so that the distinguishing traits and uses of their various organs may be ascertained, so on Professor Dewey's view the specific techniques and principles of logic receive an adequate theoretical interpretation only by being exhibited in the rôles they play within the process of inquiry.

A variant of this general principle of contextual analysis controls all of Professor Dewey's writings: it requires that process and product be taken as correlative distinctions, so that neither can be understood or assigned a status in existence independently of the other. Thus, an effective use is made of this principle in Professor Dewey's discussion of ethical issues when he maintains that the character of means employed enters constitutively into the character of ends attained. Its special application to logical theory stipulates that the conclusions of inquiry must be construed in terms of the procedures used to establish them, and that objects of knowledge, conceived as products of inquiry, can not be assigned an existence antecedently to it.

This is not a suitable occasion for marshalling evidence for the principle as a sound precept of analysis; but in any case, the apparently paradoxical features of Professor Dewey's logical theory follow from his wholehearted acceptance of it. Consider, for example, his well-known thesis that the objects of knowledge do not have an existence antecedent to inquiry. It depends on the manifestly empirical claim, which must be decided on matter-of-fact grounds, that inquiry involves a physical reconstitution of its environing situation before the latter can function as an object of knowledge. But if this claim is warranted, then it does follow with the help of the above principle that objects of knowledge can not be intelligibly assigned an existential status prior to and in independence of specific inquiries.

Professor Dewey's refusal to read the conclusions of science except in terms of the operations of inquiry, as well as his interpretation of logical principles as rules for the conduct of inquiry rather than as formulations of ontological invariants, have brought forth the accusation that his philosophy confounds questions of validity and logical order with questions of origin and development. There is perhaps only a hair-line which divides a sound application of the principle of contextual analysis from a commission of the genetic fallacy; but it does not seem to me that Professor Dewey has stumbled across it into forbidden territory. He has simply applied to matters of logic the sort of analysis prized highly in the sciences. Thus, competent students would dismiss as thoroughly incompetent any proposal to assign meanings to such propositions as that light is corpuscular or that space is curved, which neglects the complicated symbolic transformations needed before such statements can be given existential reference, or which fails to note the technological apparatus required in order to implement them with a physical sense. Refusal to heed such proposals illustrates not the genetic fallacy but scientific caution and wisdom. Professor Dewey's similar insistence that logical principles be construed in terms of the relatively stable use that is made of them in inquiry is likewise free from the fallacy; for his insistence arises from a refusal to conceive principles as if their sole identifiable habitat were textbooks and treatises, and from a conviction that it is their function in inquiry which exhibits their sole meaning and relevance. There is clearly a difference between confusing questions of origin and validity, and assigning a meaning and a degree of adequacy to propositions wholly on the basis of the circumstances of their use.

In this brief appraisal of Professor Dewey's *Logic* it is not possible to examine the details of his reconstruction of the theory of inquiry. There is one special point, however, which seems to me worth while raising. The central technical innovation of his theory is the interpretation of judgment as the settlement of an issue, so that a judgment involves the functional correlation of perceptual and conceptual material; and in elaborating this notion he introduces a set of parallel distinctions, of which the most important is the difference between generic and universal propositions. The intent of the parallel terms is to demarcate the rôles of perceptual and ideational material in inquiry, and Professor Dewey's explicit

criticism of other writers on logic frequently takes the form that they have confounded them. Generic propositions, in brief, are said to have existential import referring to spatio-temporal connections between existents, and to be contingent even though warranted by inquiry, universal propositions express necessary relations between characters or possibilities, and may be valid even if no things exist with the required traits. For example, Professor Dewey declares that "All men are mortal" is generic, if it means that all men have died or will die; but that it is universal if it means that if anything is human then it is mortal—that is to say, if it asserts a necessary relation between the characters of being human and being mortal (pp. 256ff.).

The first point to observe about this distinction is that it can not be read off from the linguistic form of statements alone and that it refers to a difference of function in inquiry. Generic propositions are thus employed to identify and isolate perceptual material, and to prepare it as evidence for or against a proposed solution of the issue at hand; universals function as statements of possible operations and their consequences, to be instituted in the interest of solving the problem (p. 274). It is therefore confusing to have Professor Dewey himself offer certain propositions as categorically illustrating one or the other of these propositional forms, for example, the Newtonian formula for gravitation as a universal proposition. If the form of a proposition is its function, is he faithful to his general standpoint in citing such formulae, taken out of their specific contexts, as examples for his distinctions? Could not inquiries be found in which this formula helps to prepare existential material in order to identify it for the sake of other operations to be performed? But in the second place, the reader is left to wonder just what is the force of "necessary" in the account of universals as involving *necessary* relations between characters. Professor Dewey's statement of his distinction, and his occasional illustrations of it, invite the identification of generic propositions with what in current literature are called synthetic ones, and of universal propositions with analytic ones; and some of his readers have already succumbed to the temptation to interpret the distinction in such an unambiguous way. Such an identification, however, simply will not do, in spite of Professor Dewey's occasional references to universal propositions as definitory in nature (e.g., p. 272). For he explicitly declares that universals

formulate possible modes of acting, so that the execution of the operation prescribed by the proposition also tests its force and relevance for solving the problem at hand. But no analytic proposition, in the contemporary technical sense, would be tested in the way frequently proposed by Professor Dewey for his universals (cf. p. 264). Moreover, he distinguishes between two sorts of universal propositions, one kind being exemplified by propositions of mathematical physics (e.g., Newton's law of gravitation), and the other by propositions of mathematics (e.g., two plus two equals four); and he recognizes that the former kind do not exhaust the possibilities, so that they may have to be abandoned under the stress of factual demands, while the latter are apparently free from such limitations (p. 398). This subsidiary distinction only aggravates the puzzle as to what is to be understood by the specification that universals assert necessary relations between characters. It follows that the whole basis of his distinction between them and generic propositions remains obscure, while at the same time the grounds for his criticism of those logicians whom he calls "formalists" are not obviously relevant. Could he not be persuaded to restate the matter with greater clarity and fullness?

In so far as I understand the distinction between generic and universal propositions, it does not seem to me to be a sharp one, though it calls attention to important stages in the continuum of inquiry. Certain propositions, suggested but not derived from empirical material, may be asserted with confidence because the conditions for warranted assertions have been approximated for them. These propositions do not enter integrally into the theoretical framework of science, and their correction or even total abandonment does not involve a radical recasting of that framework; such propositions, I am suggesting, may be Professor Dewey's generic ones. On the other hand, some propositions gradually acquire such a commanding position in the set of warranted assertions of the period (in terms of comprehensiveness and deductive power), that they reach the status of leading principles of empirical analysis and in large measure control the general direction of research in the science of which they are a part. These propositions are not easily challenged by the facts of experience, not because they have no significant alternatives, but because to challenge them would involve a fundamental overhauling of the theoretical systems of science; they

thus function as assured procedural principles, no longer at the mercy of random experiments because suitable devices are provided for obviating apparent contradictions with experience without impairing their fruitfulness as guides to further inquiry. Such propositions seems to me to fall into Professor Dewey's first kind of universals. But the propositions of mathematics differ radically from those just mentioned. They are analytic, and though also instrumental in inquiry are instrumental in a different way. For their function is to make transitions in discourse, to facilitate calculations of various sorts, rather than to direct the analysis of empirical material or to formulate possible modes of action. The recognition of these distinctions seems to me fundamental for an adequate theory of inquiry; but while Professor Dewey does not overlook them, his discussions of them and of related topics do not constitute the most enlightening portions of his treatise.

In spite of its limitations as to details, no half-way sympathetic reader of this book can lay it down without the conviction that it offers a conception of logic which has its roots firmly attached to the procedures of science, and that its integral view of the subject ranks among the great visions of the day. Those who read it must acquire courage and inspiration to contribute their share toward completing the fundamental task which Professor Dewey has envisaged with such startling clarity and adequacy.

2 PROFESSOR DEWEY'S ANALYSIS OF INDIVIDUALITY

Professor Dewey's address on "Time and Individuality"[2] argues the general thesis that the mark of individuality is development, and that "temporal seriality is the very essence of the human individual." Moreover, in opposition to the view that sequential change is foreign to the nature of inanimate things, he maintains that "the principle of a developing career applies to all things in nature." Two corollaries are drawn from this metaphysical doctrine. The first is that progress is not inevitable, but is contingent on the efforts of men to control change in a given direction, consequently, the ground of democratic ideas and practices must be a faith in the

2. *Time and Its Mysteries*. Series II. Four Lectures given on the James Arthur Foundation, New York University, by Daniel Webster Hering, William Francis Gray Swann, John Dewey, and Arthur H. Compton. New York. New York University Press. London. Humphrey Milford. Oxford University Press. 1940. viii + 137 pp. $2.00.

potentialities of individuals. The second is that art complements science in being a manifestation of individuality as creative of the future; hence, since "the free individuality which is the source of art is also the final source of creative development in time," the regimentation of artists is treason to the better future which such regimentation would serve.

Professor Dewey's essay is provocative, but his argument is puzzling. He rests his case concerning the universal validity of the "principle of a developing career" on two main contentions. The first is that "scientific objects" are purely relational, so that the method of physical science is concerned with the measurement of change and "not with individuals as such"; the second is that it is becoming clear that in physics "statements of what actually occurs are statistical in character as distinct from so-called dynamical laws that are abstract in character, and disguised definitions." Professor Dewey seems to me correct in his characterization of scientific objects. But is it really the case that the methods of the natural sciences are not concerned with "individuals as such"? Do we not have reliable physical knowledge concerning such manifest individuals as the sun, the earth, and even individual human beings? The second contention appears to me even more dubious. I will not stop to argue the question whether the theories and laws of mechanics, classical thermodynamics, relativity theory, and a good part of optics and electrodynamics are in fact statistical laws, and if not whether they are disguised definitions. I do not think that the answer is affirmative in either case. The crucial question is whether, even if the statements of physics are statistical in character, it follows that development characterizes all things in nature. It does not seem to me that this consequence can be validly drawn; for there is no contradiction in using both a statistical and a non-statistical ("dynamic") theory for the same subject-matter, as is evident from the fact that both statistical mechanics and thermodynamics can be employed in the domain of thermal phenomena.

Professor Dewey also appeals to the Heisenberg indeterminacy relation as evidence for that fact that "the individual is a temporal career whose future can not be *logically* deduced from its past," so that "for physical individuals time is not simply a measure of predetermined changes in mutual positions, but is something that enters into their being." Waiving the question whether the pro-

posed interpretation of the Heisenberg principle is adequate, it is amusing to find Professor Dewey denying that science is concerned with the intrinsic natures of things while at the same time he makes statements which do just that. Moreover, while the predictions of science are only probable, as Professor Dewey rightly maintains, this fact is not a specific outcome of the Heisenberg principle; for it is justifiable to assert that fact even for the classical dynamical theories, so long as those theories have an empirical content and are not "disguised definitions." It is not clear, therefore, what relevance the Heisenberg principle has for Professor Dewey's argument.

Professor Dewey maintains that a thing's potentialities are not fixed and intrinsic to it, but depend on the consequences of its interactions with other things. It is somewhat strange, however, to find him also maintaining that the validity of this doctrine is essentially related to a view which regards the "spatial rearrangement of what existed previously" as in some sense an inferior manifestation of development than a "qualitatively new" happening. For since we can not tell what the character of physical theories will be like after another hundred years of research, this seems like giving needless hostages to the future. But what I find really strange, in the light of Professor Dewey's main thesis, is to have him declare that an individual can lose his individuality by becoming imprisoned in routine, that "our behavior becomes predictable because it is but an external rearrangement of what went before," and that the human problem is the control of change in a given direction. Since this problem, if it is to be solved, presumably involves making predictions concerning the future on the strength of past behaviors, these various doctrines do not seem to me compatible. I do not think, therefore, that the metaphysical underpinning Professor Dewey supplies for his democratic faith is an adequate or, for that matter, a needful structure. And I think that in attempting to supply it in the manner of the present essay he has not been faithful to his own best teachings.

9

Alfred North Whitehead

1.

One of the dominant ideals of western philosophy has been the construction of a coherent, logically related system of ideas in terms of which every phase of existence can be interpreted. No philosopher in recent decades has pursued this ideal with such distinction as has Alfred North Whitehead, who died at the age of 86 on December 30, 1947. Most of the great philosophical systems of the past were developed by men intimately familiar with the sciences of their day; and in this respect also, unlike most contemporary metaphysicians, Whitehead falls within the great tradition. Indeed, it was not as a speculative metaphysician that he first achieved eminence. His *Universal Algebra* and the *Principia Mathematica* (written in collaboration with Bertrand Russell) are contributions of classic importance to mathematics and logic; and his subsequent analyses of the foundations of mathematical physics have been judged as constituting a prolegomena to every future philos-

ophy of science. It is a moot question whether Whitehead's permanent place in the history of thought is made more secure by his scientific attainments or by his essays into philosophical cosmology. But in any event, the essential features of his philosophy of organism are familiar to an audience not confined to his professional colleagues, and his system continues to influence the thinking of men in many different occupations. This unusual spectacle of a difficult philosopher with a wide circle of lay readers cannot be explained entirely in terms of Whitehead's scientific prestige, nor in terms of the grace and frequent brilliance of his prose, nor in terms of the sheer delight that his suggestive play of ideas evokes. His philosophical writings express some of the dynamic tensions of the society in which he lived, and they answer needs that are deep-seated and widely felt.

Philosophy may begin in wonder, but the wonder that rouses philosophical reflection must contain a large admixture of puzzlement. The puzzle controlling much of Whitehead's thought is produced by the apparent incongruity between the world encountered in immediate experience and the world allegedy disclosed by modern natural science. The familiar world of everyday experience is a continuum of sensuous qualities, and contains events and proccesses that are permeated with emotional and purposive values. On the other hand, the world as envisaged by an influential interpretation of classical physics consists of isolated bits of matter, shorn of all sensuous glory and stripped of any inherent direction or value. The latter world thus seems utterly alien to human experience; and if science renders a true account of the nature of things, it becomes difficult to accept the human scene as anything but an insubstantial appearance But a conception of reality according to which the traits of things most prized by men have no genuine place in it, converts human aspirations into a mockery; and worse still, it transforms the obvious efficacy of human actions into an unintelligible mystery.

It is one of Whitehead's signal achievements to have recognized that the radical bifurcation of man and nature, associated with what he called "scientific materialism," is not necessitated by physical science. He was able to show that the concepts of physics, far from describing a "real world" with which the genuine reality of things apprehended in immediate experience is incompatible, are *abstrac-*

tions referring to qualities and processes discernible in such experience. The task that Whitehead envisioned for philosophy was therefore that of supplying a critique of abstractions—a critique which would exhibit the derivation and the specific functions of abstractions in a world that contains much more than the skeletal patterns of change made explicit by natural science. But he not only proposed this task. He also developed a powerful intellectual technique for carrying it out, and used it to illumine the significance of a number of basic scientific concepts. Those who read with excitement his *Principles of Natural Knowledge*, *Concept of Nature*, and *The Principle of Relativity* when they appeared shortly after the First World War, will scarcely forget the great hope these books inspired. They were a promise of a comprehensive philosophy that could take physics seriously for its disclosures concerning the executive order of nature, and that could at the same time recognize as irreducible factors in the scheme of things the qualities and purposive activities manifested to human experience.

The books Whitehead published after he joined the Harvard faculty in 1924—especially *Science and the Modern World*, *Process and Reality*, and *Adventures of Ideas*—continued the critique of abstractions he had begun so impressively. But they continued the critique in terms of conceptions cosmic in scope, rather than by exhibiting the import of abstractions in terms of their specific functions. He now presented a comprehensive vision of the universe that appeared to integrate and illuminate an endless variety of events and processes. However, while the earlier analysis showed that the "senseless, valueless, purposeless" universe of scientific materialism is a consequence of the fallacy of misplaced concreteness, of mistaking the abstract for the concrete, Whitehead now maintained that nature can be made intelligible only if everything actual is assumed to have the traits generic to living organisms. Everything actual, according to this later doctrine, exhibits "feeling," everything actual manifests a selective activity akin to "purpose," everything actual is related essentially and "organically" to everything else. If the universe thus portrayed no longer seems alien to human aspirations, it is because the features nature displays in all her sectors are strictly analogous to features displayed by the human organism. Indeed, moral and aesthetic beauty is now claimed to be the "aim" of all existence, and not merely an occasional achieve-

ment in the human arena of the cosmos. Whitehead's culminating philosophy, like that of Plato, is thus an attempt to combine an interpretation of the universe that is both rationalistic and religious —the rationalism conceived as the faith that at the base of things there is no mere arbitrary mystery, the religion freed from superstition and institutionalized dogma. It is scarcely surprising that those seeking new foundations for ancient faiths, or rebelling against the discipline imposed by modern science on romantic speculation, have found in the philosophy of organism an answer to their needs. On the other hand, Whitehead's cosmology is a celebration of process and creative advance toward novelty as ultimate and pervasive traits of nature, and his system provides a fresh alternative to the parochial negations of many traditional patterns of thought. He was in fact sensitive as few technical philosophers have been to the movement of things and ideas, and to the subtleties and complexities of experience. The burden of much of his cosmic vision is carried by his aphoristic observation that "Life is an offensive, directed against the repetitious mechanism of the Universe." He was an extraordinarily gifted spokesman, perceptive and wise, for everything nascent, venturesome, and potentially liberating.

Whitehead's philosophic cosmology is a vision that is articulated with amazing virtuosity, but which rests less upon detailed argument than upon direct insight. While much of it is inspired by a mathematical conception of the universe, it also owes a heavy debt to the romantic tradition; and for all its stress upon the importance of time and the emergence of novelty, it has an obvious kinship with the absolute idealism of Hegel. In the hands of a great thinker disciplined by intimate contact with modern science, such as Whitehead was, the cosmology can undoubtedly be used to shed light on numerous basic problems. Nonetheless, it is at least an open question whether the systematic use, by Whitehead or lesser men, of its organismic categories as ultimate principles of interpretation for everything whatsoever, yields illumination rather than obscurity. To attribute "feeling" to whatever exists, to view all physical stresses as "appetitions," to see in the occurrence of any event the outcome of a "decision" between alternatives—what is this but to describe everything in nature in animistic terms, and to sanctify superstitions from which men have only partly freed themselves? Is it really clarifying and necessary, in order to find a place in the scheme

of things for the human scene, to describe the cosmos as if one were writing a general anthropology? Moreover, does one achieve either knowledge or a better understanding of what is already known, by simply giving new and potentially misleading names to already discriminated elements of nature—for example, by labelling all processes as having "aims"? And if we admit, as perforce we must, that no intellectual analysis, however thorough, can exhaust or reproduce the passage of concrete events, is it either well-advised or true to say that "Logic is a fake"? Such, at any rate, are some of the doubts concerning Whitehead's philosophic cosmology that besiege many questioning minds. It is not Whitehead, the speculative metaphysician, who has won the profound admiration of all his readers. The excellence many of them cherish, is the excellence found in Whitehead, the acute analyst, the steadfast critic of closed systems and provincial dogmatisms, the incredibly sensitive commentator on the diversity and the flux of existence.

2.

The pursuit of metaphysics as the study of generic characters of existence has been slowly regaining its professional adherents. Once its central theme, reaction to the unchecked flights of nineteenth century romantic speculation has well nigh banished metaphysics as a legitimate subject matter for philosophy. But the problems which professional philosophers refused to consider became acutely pressing in the special sciences. It was to be expected that ere long comprehensive treatises on the nature of existence would appear, fashioned by philosophers who were sensitive to the advances of recent science as well as to the ancient tradition that philosophy is the systematic study of being. To the series of distinguished essays on metaphysics which contemporary philosophers have contributed, these volumes[1] are a notable addition.

Process and Reality, to which *The Function of Reason* can serve as an introduction or summary, should be doubly welcome. In the first place, it is written by a man highly sensitive to the most varied phases of human experience, and exceptionally competent to write on some of them. Mathematics, physics, biology, art, religion,

1. *Process and Reality: An Essay in Cosmology*, by A. N. Whitehead. (Macmillan, 1929), $4.50.

The Function of Reason, by A. N. Whitehead. (Princeton University Press, 1929), $1.50.

are not alien to him, and in at least the first two disciplines Whitehead is an acknowledged master. As a consequence, his volumes are crammed full with insights and flashing phrases which illuminate both text and subject matter as lightning does a stormy night. In the second place, Whitehead's approach to philosophic problems has avoided traditional clichés. He has faced the issues of philosophy with the conviction that not only must the characters which science especially notes be taken as genuine traits of existence, but those specific characters which eventuate when the rest of nature interacts with the human organism must be so taken as well. The traits which appear in the segment of existence falling within human experience are taken by him as instances of, and continuous with, analogous traits in the furthest reaches of nature. His metaphysics consists, therefore, in the framing of categories adequate to describe every phase of existence, but which are nevertheless such that *any* characters whatsoever, subsumed under those categories, are shown to be not different in kind, though different in degree of complexity, from the characters manifested in human life. In this fashion does Whitehead hope to demonstrate the unity of man and nature.

No summary less extended than the book itself can do justice to the sweep and ingenuity of this cosmology, which requires one Category of the Ultimate, eight Categories of Existence, twenty-seven Categories of Explanation, and nine Categorial Obligations for its framework. In essence, however, its doctrine is a variation on the Platonic-Leibnizian theory of how a multiplicity of pure forms or eternal objects are caught up by (ingress into) the flux of actuality. For like many other metaphysicians, Whitehead finds himself in agreement with pre-Kantian fashions of thought even though he started out in opposition to them. Temporal things arise by participation in eternal things. But in coming to be a determinate instance of a pure form, a temporal thing reflects (prehends) the universe of many disjoined actual occasions, so that its selective absorption of the disjoined universe culminates in a novel and unified event, which in turn contributes its share to (becomes objectified in) the intrinsic natures of other actual occasions. Every actual entity must, accordingly, be construed as dipolar. It is something physical, the culmination of antecedent processes which emerge in it as a novel synthesis and enjoy therein a specific physical immediacy, called "feeling"; and it is also something restive or appetitive for eternal

objects as yet unrealized in the flux, thereby passing on from the finality of its own eventuation to new moments of completed creation. "Some lowly, diffused form of the operations of Reason constitutes the vast diffused counter-agency by which the material universe comes into being." Consequently, every actual entity is capable of two supplementary analyses: genetic analysis shows how the entity has become what it is as the outcome of its prehending the rest of the universe; morphologic analysis indicates the relations of efficient causality which it bears to its contemporaries and successors. The pervasive twofold character of an actual entity as physical or final, and as mental or appetitive, is the prototype selected by Whitehead in terms of which such relations as that of the mind to its body can be rendered intelligible. The initial dualism of actual entity and eternal object is mediated, however, as in Plato and Leibniz, by a God. He has both a "primordial nature," whereby the barren pure forms are eternally and ideally realized in him, and whereby he constitutes a principle of selectivity, so that the actual process never fails to exhibit the principles of Whitehead's metaphysics by precipitating new eternal objects; and he has a "consequent nature" wherein the mutual immediacy of whatever once is actual is forever conserved. So this metaphysics ends in a Bradleyan view of the universe: it is actualized in individual entities; but these are nevertheless organically connected in the unity of God's feeling of their conjoint existence, as well as through the relevance to them of every unactualized form ideally felt by God.

In spite of the speculative power of this cosmology, it will leave some readers dissatisfied and censorious. This reviewer, at any rate, must protest, in the first place, against the severe abuse of language to which Whitehead is partial. Every attempt at precision in metaphysics is praiseworthy. But when, instead of using newly coined words or old words carefully redefined, he introduces words that have an accepted meaning and an inevitable human association to denote his own unusual categories, he invites confusion and obscurantism. Words like "feeling," "satisfaction," "decision," "appetition," "society," which have a vague enough accepted connotation, can be employed only with danger as the equivalents, more or less, of more neutral words like "immediacy," "completion," "selectivity," "incompleteness," "structure." For the ease with which the former set of words become symbols for specifically human qualities,

which are then read into every occurrence in nature, makes them unfit for exact thought, especially when their meanings are loosely stated. It is a strange irony that the co-author of *Principia Mathematica* should have at once so great a distrust of language that he disowns it as adequate to express propositions, and yet so much faith in a word like "feeling" that he uses it to connote indiscriminately the different types of immediacy which events possess. If language does not "express" a proposition, what, one would like to know, does? What is "expression" if language does not do it, and what is a "proposition" if language does not adequately express it? The program to construct a coherent, logical, and necessary system of ideas, in terms of which every element of experience is to be stated, is not far from sheer verbalism if, for example, every variety of immediacy is subsumed under the label of "feelings," and if thereby the tenuous analogy between the feelings of human beings and the specificity of any event is expanded into an identity of type.

Speculation, as Whitehead urges, is the life blood of science and philosophy. But he also admits that it must not run wild. Unless the speculative flight ends in theories which are directly or indirectly verifiable in human experience, what differentiates a daring philosophy from myth and superstition? Now what are the possible ways of verifying Whitehead's ubiquitous feelings, satisfactions, unconscious prehensions, or his God's patience, wisdom, consciousness, and conservation of all values? Either these terms do not mean what they ordinarily mean, and then this metaphysics must certainly be supplemented by more analytic studies; or the existence of what they denote is not verifiable. Furthermore, when Whitehead declares that by the "coherence" of ideas he understands that no entity be conceivable in "complete abstraction" from the system of the universe, one may retort that no relational way of thought can declare itself otherwise. But one must also add, that while there can be no "complete abstraction" in this sense, there undoubtedly is a "relative abstraction" so that one must not make the impossible demand that the nature of the whole universe be presupposed in whatever we may say. Whitehead's pursuit of truth as "nothing else" than how the composite natures of actualities receive representations in God's nature (p. 18), is fortunately not his only occupation, otherwise the *Universal Algebra* and the *Principia* would never have been written. And when one reads,

further, that the "necessity" of a philosophic scheme must bear in itself its own warrant of universality (p. 5); or when a frequency theory of probability is made to depend on an intuitive and infallible knowledge of "equal probabilities" (p. 307); the reader can only gasp and wonder at the audacity of the mathematician who denies the principles of the method of his own science when he turns philosopher.

More serious difficulties in this scheme of ideas remain. This cosmology is an explicit attempt to combine a pluralism of events, an appreciation of their uniqueness and contingency, with an organic view of existence in which the "togetherness" of things is facilitated by God's non-temporal conceptual feeling of the realm of pure forms. In so far as each thing is what it is in virtue of the concrescent union in it of other things, existence is organically connected and all relations are internal. Nevertheless, Whitehead recognizes two types of prehensions, a positive kind, in which a "positive," definite bond exists between every pair of items in the universe, accompanied by a feeling of that bond; and a negative kind, in which some items are excluded from contributing to the subject's constitution. All actual entities, but only a selection of eternal objects, are felt by a given subject; nevertheless, even the eternal objects *not* felt play a rôle, since negative prehensions too have their immediate aspects, and the "negative prehension of an entity is a positive fact with its subjective form" (p. 66). The proverbial flea on the dog's tail does add, apparently, to the actual nature of the dog-star; for if it does not do so "positively," the exclusion of the flea from contributing to the internal constitution of Sirius is nevertheless accompanied by an appropriate subjective feeling all the same. The "organic" nature of existence is proved because all things must be related, since if two things are not related in a specific way, the non-relation is, after all, a relation of a kind! All this may be true, but the conclusion that existence is organic is won at the price of so far diluting the meaning of "organism" that the application of the term to specific biological structures no longer differentiates the latter from "inorganic" structures. Of what avail to call the interrelation of the stars organic, if the kind of organization the stars exhibit is poles apart from the organization of living things?

Why anything should happen at all, why actual things should be

"restless after novelty," is a problem which no naturalistic philosophy need face, since if it is faced it must be left unanswered, even if it is left in the bosom of God. Whitehead's philosophic writings have been largely devoted to calling attention to this element of ineradicable freshness and unaccountability in every event, and he has not hesitated finally to identify this creative urge with the divine element in actuality. The price of novelty, however, is the passing away of characters which antecedent events possessed, and the creative advance of nature is accompanied by the perishing of those immediacies which have already emerged. On Whitehead's theory of God's twofold nature it is difficult to see how creation does retain its spontaneity, and how what is once actual does pass away. For the primordial nature of God is his feeling the entire multiplicity of eternal objects, so that by reason of God's sharing in the nature of actual entity, there results a gradation of the relevance of eternal objects to the career of that entity. "Effective relevance requires agency of comparison, and agency belongs exclusively to actual occasions. This divine ordering is itself matter of fact, thereby conditioning creativity. Thus possibility which transcends realized temporal matter of fact has a real relevance to the creative advance" (p. 46). That individuality and self determination which appeared to be the contribution of every event to existence, seems here to be derivative from the primordial nature of God which transcends the given actual occasion. On the other hand, the consequent nature of God provides for the objective immortality of every actual thing. "In it there is no loss, no obstruction. The world is felt in a unison of immediacy" (p. 524). God thus becomes the great companion, the fellow sufferer who understands! But either the "unison of immediacy" is a Pickwickian description of the *posterity* or causal consequences of actual entities, or death and decay become illusions.

In Whitehead's system, therefore, the possibilities into which actual entities can develop are given once for all, and unlike Charles Peirce's cosmogenic growth of possibilities, novelty in actuality must always come from "somewhere." Whitehead's conclusion is in line, therefore, with the traditional philosophic and theologic systems. But one reader at least would like to know what is the literal meaning of conceptual prehensions of eternal objects, if consciousness is not necessary for such prehensions. One would like to know on the basis of what evidence, or upon what interpretation, ap-

petition for an eternal object plays the rôle of final cause, efficacious in the realization of determinate occasions. One would like to know Whitehead's *detailed* solution of the well known antinomies arising from the notion of the class of abstracta (p. 73). One would like to know what it means to say that "apart from the experiences of subjects there is nothing, nothing, nothing, bare nothingness" (p. 254), or that it is the appetition *for* unrealized forms which is the final cause of the temporal process One must ask whether the characterization of process as an "influx" of eternal objects, or of change as the description of the adventures of eternal objects in actuality, are really contributions to an understanding of change and process. One suspects, therefore, that eternal objects, instead of being regarded as the relational patterns invariant over segments of existence, are uncritically hypostatized so that a God must be invoked to precipitate them back into the flux. And one suspects that Whitehead has accomplished the resolution of the Cartesian problem of how the mind and body interact, only by raising the hopeless question of how possibilities can be effective in regulating their own realization.

10

The Philosophy of Bertrand Russell

RUSSELL'S WRITINGS on the philosophy of science exhibit one persistent feature: his explicitly avowed use of the maxim "Whenever possible, substitute constructions out of known entities for inferences to unknown entities,"[1] a maxim which elsewhere he calls "the supreme maxim in scientific philosophizing." Acting upon this precept, he has attempted to show that the ostensible objects of science are "logical fictions," capable of definition in terms of ap-

1. *Contemporary British Philosophy*, First Series, edited by J. H. Muirhead, London and New York, 1925, p. 363. Subsequent references to this book will be abbreviated to *CBP*.

In this essay the following abbreviations will be used for the titles of books by Russell. *FG* for *An Essay on the Foundations of Geometry*, Cambridge, England, 1897, *IMP* for *Introduction to Mathematical Philosophy*, London and New York, 1920; *P* for *Philosophy*, New York, 1927, *AM* for *The Analysis of Matter*, London, 1927; *OKEW* for *Our Knowledge of the External World*, Second Edition, New York, 1929, *PM* for *The Principles of Mathematics*, Second Edition, New York, 1938, *ABC* for *The ABC of Relativity*, London, 1925; and *IMT* for *An Inquiry into Meaning and Truth*, New York, 1940.

propriately selected elements. I wish in this essay to examine the type of analysis which Russell has brought to bear upon the logical problems of physics as a consequence of his adoption of this maxim. However, Russell has repeatedly called attention to the fact that it was the fruitfulness of certain logical techniques in the foundations of mathematics which led him to adopt the maxim as the supreme guide in philosophy. I shall therefore briefly consider those techniques, as they are employed in the context of Russell's reconstruction of pure mathematics, preliminary to the discussion of his analysis of physics.

I.

1. Russell's by now classic studies on the foundations of mathematics brought to a conclusion what was, at the time of their publication, a revolution in traditional conceptions of mathematics. As is well known, the explicit thesis for which those studies supply overwhelming evidence is the essential identity of logic and pure mathematics. In exhibiting that identity, Russell also establishes the untenability of certain influential theories of knowledge which were based upon historically widespread views as to the nature of mathematics. For by clearly distinguishing between pure mathematics, whose propositions contain only logical terms, and applied mathematics, whose propositions contain descriptive (or empirical) as well as logical terms, he cut the ground from under the claims of dogmatic rationalism, Kantian apriorism, and types of sensationalistic empiricism. On the other hand, Russell's own analyses seemed to require the adoption of an extreme form of Platonic realism, since his detailed justification of mathematics as a body of valid propositions appeared to be cogent only on the hypothesis of the "independent reality" of universals and relations. Indeed, it was in considerable measure because of this supposed connection between such a realism and Russell's major thesis about mathematics, that the logico-symbolic techniques he employed so brilliantly were believed to require definite philosophical commitments, so that the use of those techniques became the center of philosophic controversy.

Nevertheless, some of Russell's most notable achievements in the analysis of mathematical notions exemplified a tendency opposed to Platonic realism. His analysis of the notion of cardinal number, for example, showed that it was unnecessary to assume the "exist-

ence" (or "subsistence") of a specific type of entity to correspond to the notion; and, accordingly, he showed that, without affecting the structure or validity of mathematics, the "ultimate population" of Platonic objects may be supposed to be smaller than had been thought.

In effecting such economies, Russell was in fact carrying on a great tradition in mathematics. Thus, the "extension" of the number-concept in the history of mathematics was first accompanied by the postulation of special *kinds* of number (the rational fractions, the signed numbers, the irrationals, the imaginaries, the infinitesimals, and so on) to serve as the objects "discovered" by mathematicians. But the subsequent work of such men as Hamilton, Von Staudt, and Weierstrass made it evident that the postulation of such numbers as distinctive sorts of entities was unnecessary, since the required "entities" can all be defined in terms of familiar arithmetical notions and operations. Accordingly, when Russell declared

> Every one can see that a circle, being a closed curve, cannot get to infinity. The metaphysician who should invent anything so preposterous as the circular points [at infinity], would be hooted from the field. But the mathematician may steal the horse with impunity,[2]

and when, years later, commenting on the mathematician's occasional practice of postulating what is required, he noted that "The method of 'postulating' what we want has many advantages; they are the same as the advantages of theft over honest toil,"[3] he was doing less than justice to the tendency which the history of mathematics illustrates of eventually supplanting dubious "inferences" by suitable "constructions." Russell's maxim of philosophizing simply makes explicit a long-range trend of mathematical development.

2. For the sake of definiteness, the operation of Russell's techniques for avoiding needless postulations in mathematics will be illustrated in three cases. First, the cardinal numbers. The cardinal numbers are generally admitted to be predicable of classes, two classes being assigned the same number when they are similar (i.e., when their members can be correlated in a one to one fashion). It seems natural, therefore, to regard the cardinal number of a class as the property which that class has in common with classes similar to it; and, on this view, a cardinal number is sometimes said to be

2. *FG*, 45-6.
3. *IMP*, 71.

obtained "by abstraction" from the classes possessing it. However, there seems no good reason for supposing that similar classes have *just one* property in common rather than a *set* of properties. There is even room for doubt whether at least one such property "exists"; for in assuming the existence of such a property we are assuming, according to Russell, "a metaphysical entity about which we can never feel sure that it exists or that we have tracked it down."[4] In order to avoid these difficulties he therefore defined the number of a class as the *class* whose members are classes similar to the given class. Since it can be proved that there is only one such class of classes, the first difficulty vanishes; and since this class possesses all the formal characteristics expected of cardinal numbers, while at the same time its existence is "indubitable," it no longer is necessary "to hunt for a problematic number which must always remain elusive."[5]

Consider next Russell's definition of the real numbers, for example, of the irrational number which is the square-root of two. It is well known that the square-root of two is not an integer and that it cannot be a rational number. What, then, is it? Prior to Russell's analysis it was customary to regard it as the *limit* of certain series of rational numbers, or more generally, as a distinct kind of entity whose "existence" was assumed for the sake of satisfying certain mathematical relations. For example, the rational numbers, if ordered according to magnitude, form a series. In many cases this series can be decomposed into two ordered classes, such that one of the rational numbers separates their members; thus, the two classes, rationals less than two-thirds and rationals greater than two-thirds, are separated by the rational number two-thirds. On the other hand, consider the two ordered classes of rationals, rational numbers whose squares are less than two and rational numbers whose squares are greater than two; in this case, no rational number effects the separation. It again seems "natural" to suppose that there must be a number, though not a rational one, which "lies between" these two classes. But what cogent grounds have we for assuming the "existence" of such a number? Russell argued that we have none, and that it is only the influence of irrelevant spatial imagination or the seductiveness of certain algebraic operations which lends an air

4. *IMP*, 18.
5. *IMP*, 18.

of plausibility to such an assumption. The assertion of the "existence" of a new kind of number is thus an unwarranted "inference," and introduces something problematic and elusive into mathematics. On the other hand, the existence of the *class* of rationals whose squares are less than two is *not* disputable, for this class is "constructed" out of "known" elements. Accordingly, since the mathematical properties usually attributed to irrational numbers can be shown to belong to that class, Russell defined the square-root of two as identical with that class of rational numbers.

The notion of class plays a fundamental rôle in these two examples. But, according to Russell, classes, like cardinal and real numbers, are not part of "the ultimate furniture of the world" (since they are neither "particulars" nor properties or relations of particulars), and must thus be regarded as "logical constructions." He therefore required a definition of classes

> which will assign a meaning to propositions in whose verbal or symbolic expressions words or symbols apparently represting classes occur, but which will assign a meaning that altogether eliminates all mention of classes from a right analysis of such propositions. We shall then be able to say that the symbols for classes are mere conveniences, not representing objects called "classes," and that classes are in fact . . . logical fictions.[6]

Russell achieved his objective by devising as translations for statements explicitly about *classes* other statements which mention only certain *properties* possessed by the individuals that would ordinarily be said to be members of those classes. Before illustrating Russell's procedure, a certain difficulty in effecting such translations must be mentioned. A given property (e.g., being human) determines uniquely just one class (i.e., the class of men); but the same class will be determined by two or more non-identical properties, if those properties are formally equivalent—that is, if every individual which possesses one of the properties also possesses the other, and conversely. Thus, the two non-identical properties of being human and being a featherless biped determine the same class. Hence, in order to effect the desired translation of a statement about a class, some device must be introduced so that in the new statement no one special property is mentioned *in exclusion* of other properties which also may determine the class in question. With this explana-

6. *IMP*, 181f.

tion in mind, but omitting fine points, Russell's general procedure may be illustrated as follows. The statement "The class of points in a plane is as numerous as the class of lines in a plane" is explicitly about two classes, one of which is certainly determined by the *property* of being a point in a plane and the other by the *property* of being a line in a plane. The approximate translation proposed for this statement is:

> There exist at least two properties such that one of them is formally equivalent to the property of being a point in a plane, the other is formally equivalent to the property of being a line in a plane, and such that for every individual which has the first property there is just one individual with the second property, and conversely.

Although for the statement here chosen a somewhat simpler translation of the requisite kind can be given, the indicated translation illustrates the sort of complexity which Russell believed is required in general. In any event, the proposed translation makes no mention of any classes; and, accordingly, the assumption that classes "exist" as special kinds of entities is not required.

Let us finally state what appears to be the general pattern of the procedure of substituting "constructions" for "inferences." Let "S_1" be a statement, employed in some definite context T_1, which contains explicitly the expression "C," where this expression symbolizes some entity *C*; that is to say, "S_1" would normally be supposed to be about *C*. Under what circumstances is *C* (the *entity*, not the *expression* "C") to be regarded as a "logical construction" or "logical fiction?" Suppose there exist a set of entities a_1, a_2, a_3, . . . , and a set of relations R_1, R_2, . . . ; suppose, further, that a statement "S_2" can be formed which contains mention of these entities and relations but does not contain the expression "C"; and suppose, finally, that in the context T_1 the statement "S_2" is logically equivalent to "S_1." If these conditions are satisfied, *C* is a logical construction out of the specified entities and relations. It will be noted that the above three examples conform to this schematism. It is clear that the statement "S_2" cannot, in general, be obtained from "S_1" by simply replacing "C" in the latter by a more complex expression without altering the rest of "S_1"; the formulation of "S_2" involves, in general, a radical recasting of "S_1."

3. A number of observations can now be made on Russell's use of his maxim of philosophizing in his reconstruction of mathematics.

Although Russell substitutes "constructions out of known entities for inferences to unknown entities," he can maintain an attitude of theoretical neutrality with respect to the existence or non-existence of such things as numbers and classes. As he himself says, "When we refuse to assert the existence of classes we must not be supposed to be asserting dogmatically that there are none. We are merely agnostic as regards them: like Laplace, we can say, '*je n'ai pas besoin de cette hypothèse.*'"[7] The maxim thus expresses a principle of caution and economy, and the techniques which implement it cannot by themselves help to decide what "exists" and what does not. Those techniques assume that certain entities and relations are in some sense "given."

It is well known, however, that in developing a mathematical system there is usually considerable leeway as to what materials may be taken as primitive and what is to be defined. From a formal point of view, the characterization of something as a "construction" must always be viewed as *relative* to the base selected. Accordingly, it seems as correct to regard the cardinal numbers as *primitive* (relative to a system, like Hilbert's, in which certain concepts of logic as well as of mathematics proper are taken as basic) as it is to regard them as *constructions* (relative to a system, like Russell's, in which concepts of logic are the sole primitives). Which base is in fact adopted will in general depend on matters that are not exclusively logical: upon issues of technical efficiency, upon certain more inclusive practical requirements, and often upon antecedent commitments as to what is "metaphysically" or "epistemologically" ultimate. From such a formal point of view, Russell's reconstruction of mathematics is primarily the systematization of a large body of propositions, in which remarkable economy is achieved in showing the various relations of dependence between different portions of mathematical doctrine; and Russell's claim that the concepts and propositions of general logic are sufficient for developing formally the rest of mathematics is hardly debatable. But seen in this light, his technique for avoiding needless postulations is simply one device among others for attaining a maximum of inclusiveness and generality with a minimum of special assumptions. From this point of view at least, the issue he frequently raises as to whether numbers

7. *IMP*, 184.

and classes "exist" in some ontological sense does not appear to be relevant to the problem under consideration.

On the other hand, Russell's reconstruction of mathematics may also be viewed as an attempt to *analyze* mathematical notions so as to exhibit their relevance to everyday affairs and science. It is this point of view which is paramount when Russell declares that that sort of definition of cardinal number is required which will make possible the "interpretation" of statements like "There were twelve Apostles" or "London has six million inhabitants." And he maintains that his logical definition of the cardinals "makes their connection with the actual world of countable objects intelligible."[8] Accordingly, the fundamental issue which arises in this connection is whether Russell's analyses state what is "meant" by mathematical expressions, not simply in the context of the formal development of mathematics, but in the context of statements about the empirical world; in other words, the issue is whether Russell's analyses explicate the *use* of mathematical expressions in the context of procedures such as counting and measuring.

Unfortunately, Russell does not always keep this issue at the center of his concern, and as a consequence it is often most puzzling to know just what he is doing when he says that he is "defining" the various concepts of mathematics. Thus, in commenting on the definition of cardinal number which he and Frege developed, he declares:

> The real desideratum about such a definition of number is not that it should represent as nearly as possible the ideas of those who have not gone through the analysis required in order to reach a definition, but that it should give us objects having the requisite properties. Numbers, in fact, must satisfy the formulae of arithmetic, any indubitable set of objects fulfilling this requirement may be called numbers.[9]

Russell is surely right in saying that a definition of number need not reproduce the "ideas" of those who use numerical expressions, since most people do not know *how* they use them. But it seems to me a serious blunder to maintain that "any indubitable set of objects" which satisfy the formulae of arithmetic may be called numbers—*if* the business of "logically constructing" numbers is to be something other than a purely formal exercise, and *if* the re-

8. *PM*, Introduction to the Second Edition, vi.
9. *OKEW*, 222.

sultant analyses are to express the way or ways in which "number" is employed. From the point of view of the present approach, it is important to bear in mind the observation that an analysis or "logical construction," which is adequate for one context in which an expression is used, is not necessarily adequate for another context, and is unlikely to be adequate for all contexts. It does not follow, therefore, that definitions of the various numbers which are suitable for developing mathematics formally and systematically are suitable as analyses of them in other domains where they are used.

Two special difficulties which aggravate the analysis of mathematical concepts are worth noting in this connection. In the first place, many mathematical expressions are employed only within some more or less formalized system of mathematical statements, and have no clear or direct connections with statements which formulate matters in the actual world. The *use* of such expressions within the symbolic system may be governed by fairly explicit rules of operation, although no interpretation for those expressions may be feasible which would make the latter symbolical of anything known to occur in any part of the environment. In other words, such expressions may have an important function within what may be called a "calculus," without being "in themselves" in any way "representative." Many students (like Hilbert and Hermann Weyl) have accordingly eschewed the doubtless "natural" desire to interpret them in terms of something familiar, and have been content to exhibit the rôles which specific calculi (containing such expressions) play in the system of scientific formulations. In any event, the interpretation of such expressions as denoting entities, allegedly "constructed" out of "indubitable" elements, appears to be a gratuitous enterprise. The second difficulty is that in actual practice many mathematical expressions have no *precise* use, however precisely they may be defined in terms of the basic notions of a formalized system. Analyses of what such expressions "mean," when such analyses yield something "constructed" in a precise way out of definite things or operations, must therefore be viewed as *proposals* as to how those expressions *might* be used. A proposal, however, is not to be judged in terms of truth or falsity, but in terms of its convenience and effectiveness in achieving specific objectives. And if Russell's definitions are such proposals, as I think some of them are, the issue he raises with respect to them, whether

the "entities" corresponding to them are "inferred" or "constructed," does not appear to have much point.[10]

One final observation. If an entity is a logical construction, then a symbol representing that entity is theoretically capable of elimination from any statement in which that symbol occurs. It has already been noted that, if an entity can be shown to be a logical construction, considerable economy can be effected in developing mathematics. However, it is also worth noting that a gain in economy in one detail may have to be bought at the price of complicating the structure of mathematics in other details—perhaps even at the price of requiring dubious assumptions concerning "the ultimate furniture of the world." Now in fact some of Russell's definitions, when these are taken as exhibitions of the structure of mathematical objects in terms of "indubitable" elements of the "actual world," do seem to me to have this dubious character. If the "existence" of a real number is doubtful when it is conceived as a special *kind* of thing, is its "existence" better warranted when it is identified with an *infinite series* of rationals? Again, Russell is not sure that classes "exist." But, in his translations of statements ostensibly about classes, he does not hesitate to introduce existential quantifiers with respect to *properties*—a procedure which requires him to assume the existence of an indeterminate *range* of properties. Is this assumption, construed in the "realistic" fashion that Russell adopts, so obvious that it may safely be taken as a metaphysical foundation for mathematics? I am not suggesting that Russell's definitions are not adequate for the purposes of systematizing formal mathematics; and fortunately I am not required on this occasion to propose a more satisfactory "metaphysics" for mathematics than his. I raise these questions only to call attention to the complex issues which await

10. Whether Russell's definitions of the various kinds of numbers do explicate the use which is made of the latter in everyday affairs and science, is a highly debatable question. I think that his definitions of the specific finite cardinals do express satisfactorily at least part of what is involved in the use of such statements as "I have ten fingers," "There is only one even prime," or "New York is more than 200 miles from Boston," although I am less sure than he appears to be that certain *ordinal* notions are not involved in that use, as Norman R. Campbell and Hermann Weyl have suggested. On the other hand, I am quite unconvinced that Russell's analysis of the irrationals is the appropriate one for "interpreting" such statements as "The diagonal of this square is equal to the square-root of two inches." For, although in explicating the sense of such a statement reference to a set of rational numbers is required, I do not think that this reference is to an *infinite series* of rationals.

us when we employ his supreme maxim of philosophizing in a metaphysical rather than a methodological spirit.

2.

1. Russell's concern with the positive sciences is dominated almost exclusively by "the problem of the relation between the crude data of sense and the space, time, and matter of mathematical physics."[11] Like many of his contemporaries, he has been impressed by the highly abstract character of physical theory, and by the *prima facie* difference between the manifest traits of the world which are exhibited in our daily experience with it and its constitution as reported by the theoretical sciences. The theories of classical physics already provided ample materials for embroidering this difference; those theories employed such notions as that of instantaneous velocities, point-particles, mathematically continuous motions, and perfectly rigid and elastic bodies, although there appears to be nothing in our common experience to which these notions are applicable. But it was the advent of relativity theory and quantum mechanics, with their novel geometries and chronometries and their revolutionary conceptions of matter and causality, which supplied the chief stimulus to Russell's preoccupation with the problem.

However, the "critique of abstractions" for which the problem apparently calls may take several different forms. Russell's conception of the task of such a critique is controlled entirely by his view that the familiar concrete objects of daily life, no less than the abstract and remote entities of theoretical physics, are logical constructions. His approach to the problem must be clearly differentiated from so-called "operational" or "functional" analyses of scientific concepts—analyses which take "common-sense" knowledge and "common-sense" objects for granted. Something must therefore be said at the outset about the general pattern of Russell's views.

Like most philosophers, Russell believes that any discussion of the relation between theoretical physics and experience starts with admitting the familiar facts of common knowledge. But he maintains that on the one hand this knowledge is vague, complex, and inexact, and that on the other hand some types of its "data" are more certain and more "indubitable" than others. In order to obtain a secure

11. *OKEW*, viii.

foundation for knowledge we must therefore separate out those beliefs which are "inferred" from or "caused" by other beliefs, from the beliefs which are both logically and psychologically prior to all others. The "hardest" or "most certain" of all data (that is, data which "resist the solvent influence of critical reflection") are the truths of logic and the particular facts of sense.[12] The logical starting point of a philosophical inquiry into physics must therefore be with our immediate, direct perceptions. The problem of the relation of theoretical physics to the facts of experience can therefore be amplified as follows:

> The laws of physics are believed to be at least approximately true, though they are not logically necessary, the evidence for them is empirical. All empirical evidence consists, in the last analysis, of perceptions; thus the world of physics must be, in some sense, continuous with the world of perceptions, since it is the latter which supplies the evidence for the laws of physics. . . .
>
> The evidence for the truth of physics is that our perceptions occur as the laws of physics would lead us to expect—e.g., we see an eclipse when the astronomers say there will be an eclipse. But physics never says anything about perceptions, it does not say that we shall see an eclipse, but something about the sun and the moon. The passage from what physics asserts to the expected perception is left vague and casual; it has none of the mathematical precision belonging to physics itself. We must therefore find an interpretation of physics which gives a due place to perceptions, if not, we have no right to appeal to the empirical world.[13]

Russell's problem has therefore a two-fold aspect. One phase of it consists in finding an "interpretation" for physics which will make its propositions relevant to the crude materials of sense; and, as will appear, this concern leads Russell to adopt the view that all the objects of common-sense and developed science are logical constructions out of *events*—our perceptions being a proper sub-class of the class of events. The other phase of the problem consists in justifying the truth-claims of physics; and this concern leads Russell to examine what data may serve as the most indubitable foundation for our knowledge, and to a discussion of the causal theory of perception as the ground for assuming the existence of events that are not perceptions. The two aspects of the problem are not independent, since the resolution of the second depends in part on

12. *OKEW*, 75.
13. *AM*, 6-7.

the answer to the first, whereas the first requires that the "indubitable entities" (which is the business of the second to specify) are already available. However, in the remainder of the present section I shall briefly examine some of Russell's views on perceptive knowledge; the discussion of his analysis of scientific objects will be left for the final section.

2. According to Russell, the original datum of experience consists of perceptions which are held to be known "non-inferentially"; included in this original datum are such items as specific shapes and colors, and relations like something being earlier than something else or something being above something else. Common-sense objects like tables and books, on the other hand, must be regarded as in some sense "inferred." They are said to be "inferred," not because we have actually inferred them, but because our knowledge of them rests upon correlations between perceptions. These correlations are not invariable, and since we may be led to entertain false expectations by relying on them we do not "genuinely know" common-sense objects.[14] The proper comment upon this conclusion, so it seems to me, is to insist that we sometimes *do* know physical objects like tables and chairs, in a perfectly good and familiar sense of "know," in spite of the fact that we may sometimes be deceived about them. But this is not the issue I now wish to raise, important though it is. The question I want to put is whether, in distinguishing between perceptions as primitive and physical objects as derivative from perceptions, Russell is doing logic or psychology. Russell's *problem* certainly requires the distinction to be one of logic, for his aim is to *define* physical objects in terms of sensory qualities. From this point of view it is clearly *irrelevant* whether in the genesis of our knowledge the apprehension of discrete sensory qualities comes before or after the apprehension of configurations of qualities. Russell himself frequently makes it plain that it is not questions of psychology with which he is concerned.[15] Nevertheless, he also says that the primitive data of knowledge must not only be logically but also psychologically prior to the knowledge he regards as derivative. Thus, he declares that the "space"

14. *AM,* 186.

15. See, for example, his quite explicit statement on this point in his "Professor Dewey's 'Essays in Experimental Logic,'" *The Journal of Philosophy,* Vol. XVI (1919), 8 ff.

into which all the percepts of one person fit is a "constructed space, *the construction being achieved during the first months of life.*"[16] And here Russell is obviously talking psychology. However that may be, the empirical evidence drawn from modern psychology is certainly unfavorable to the notion that perceptions are psychologically primitive. On the contrary, that evidence supports the view that sensory qualities and relations are obtained only as the end-products of a deliberate process of discrimination and analysis, a process which is carried on within the framework of a "common-sense" knowledge of physical objects.

What reasons are there for regarding perceptions as the most indubitable data of knowledge? As far as one can ascertain, Russell rests his case on the simple dictum that what is more primitive is also the more certain. Thus, he asserts that

> When we reflect upon the beliefs which are logically but not psychologically primitive, we find that, unless they can on reflection be deduced by a logical process from beliefs which are also psychologically primitive, our confidence in their truth tends to diminish the more we think about them.

And he concludes that "There is . . . more need of justifying our psychologically derivative beliefs than of justifying those that are primitive."[17] Why should this be so? Russell's answer is: because the derivative beliefs are non-demonstratively "inferred" from the primitive ones and are therefore less certain than the premises from which they are drawn, and because a belief is the more certain the "shorter" is the causal route from the cause of a belief to the belief.[18]

These views seem to me to rest on unsatisfactory evidence. Russell calls those data "hard" which resist the solvent influence of critical reflection. But in order to undertake such reflection, it is necessary to employ *some* principles in terms of which the attribution of "hardness" to specific data is to be evaluated; and such principles, if their authority is to count for anything, must be better warranted than the materials under judgment. However, such principles can themselves be warranted only by the outcome of our general experi-

16. *AM*, 252, italics not in the text.

17. *OKEW*, 74-5.

18. *IMT*, 164, 200. He also says: ". . . A given reaction may be regarded as knowledge of various different occurrences. . . . The nearer our starting point [in the process leading to a certain event in the brain] is to the brain, the more accurate becomes the knowledge displayed in our reactions." *P*, 132.

ence, and their certainty—of whatever degree this may be—cannot therefore be a consequence of their being psychologically primitive. Russell's entire argument, moreover, is based on a principle of reasoning which I find most debatable—the principle that the conclusion of a non-demonstrative inference cannot be more certain than any of its premisses. Quite the contrary appears to be the case in general. To take a simple illustration, if a number of witnesses testify to the occurrence of some event, the proposition that the event did occur may be more certain than any single item in the testimony, provided those items are independent. It is indeed partly in terms of the principle embodied in this example that the credibility of scientific theories is augmented. And if one accepts it as generally valid, little ground remains for the view that our psychologically primitive beliefs are also our most certain ones.[19]

Russell is not unaware of how difficult it is to identify primitive, "non-inferred" data. Thus, he notes that the records of any observation or experiment always involve an "interpretation" of the facts by the help of a certain amount of theory. He also acknowledges that "perceptions of which we are not sufficiently conscious to express them in words are scientifically negligible; our premisses must be fact which we have explicitly noted."[20] And elsewhere he insists that "a form of words is a social phenomenon," so that a person must know the language of which it is a part, as well as be exposed to certain stimuli, if he is to make true assertions.[21] The admission of the socially conditioned character of significant perception would normally be considered as a good ground for rejecting the view that perceptions are psychologically primitive. Nevertheless, Russell believes that it is possible to whittle away the element of interpretation in perceptive knowledge, and that "we can approach asymptotically to the pure datum."[22] But if pure data can be reached only asymptotically—and that means they are never *actually* reached—why is it important to try to base all our knowledge upon them? Moreover, Russell admits that some "interpreta-

19. On some of the difficulties in the view that the "shortness" of the causal route between a belief and its cause can be taken as a measure of the certainty of the belief, see my "Mr. Russell on Meaning and Truth," *The Journal of Philosophy*, Vol. XXXVIII (1941), and reprinted in this volume.

20. *AM*, 200.

21. *P*, 262.

22. *IMT*, 155.

tions" which accompany perceptions "can only be discovered by careful theory, and can never be made introspectively obvious"; and he thinks that such interpretations, at any rate, "ought to be included in the perception."[23] One cannot therefore help asking: If our actual data involve an element of "interpretation" and "inference," how in principle can we exclude physical objects as objects of knowledge on the ground that physical objects involve an element of "inference"? The distinction between the primitive and the "inferred" certainly shows the mark of being irrelevant to a working epistemology.

In any event, by his mixing up questions of logic with those of psychology Russell compromises at the very outset his program of exhibiting common-sense and scientific objects as logical constructions. That program presumably requires the analysis of these objects as structures of elements which are experientially accessible. If such an analysis is to be more than a formal logical exercise, those elements cannot simply be *postulated* to exist; and Russell's psychologically primitive "pure data" apparently have just this status.

3. Russell introduces another distracting confusion when, in order to establish the importance of regarding physical objects as constructions, he argues the case for an epistemological dualism and against "naïve realism." The truth or falsity of epistemological dualism does not seem to me germane to the question whether physical objects are analyzable into structures of specified entities. I shall therefore comment only briefly on the following views central to Russell's epistemology: that our percepts are located in our brains; that the causal theory of perception is the ground for inferring the existence of unperceived events; and that our knowledge of physical objects is "inferred" from percepts in our brain.

Russell maintains that, although it may be natural to suppose that what a physiologist sees when he is observing a living brain is in the brain he is observing, in fact "if we are speaking of physical space, what the physiologist sees is in his own brain."[24] This seems to me incredibly wrong if the word "see" is being used in the ordinary sense in which we talk about seeing a physical object; and it is this ordinary sense of the word which Russell is employing when he supposes a physiologist to be observing a brain. There might indeed

23. *AM*, 189.
24. *P*, 140.

be a sense of "see" in which I see my own brain, though I have not the slightest inkling as to what that sense is. I do know, however, that I have never seen any portion of my own brain, and that I have seen many physical objects—where the statement that I have not seen one but seen the other is to be understood in the customary sense of "see." To deny the facts expressed by the statement seems to be absurd; and such a denial can be understood only if we suppose that the person making the denial is misusing language. Moreover, such facts seem to me basic for every sound epistemology and every sound interpretation of science; and, however difficult it may be to do so, the findings of physics and physiology must be interpreted so as to square with them.

The evidence Russell offers for the causal theory of perception derives whatever plausibility it has from the tacit assumptions of common-sense knowledge; accordingly, it is not this theory which can justify such common-sense assumptions as that our perceptions may have unperceived causes. Russell's chief argument for that theory consists in showing that if we accept the theory we can formulate the course of events in "simple causal laws." For example, he declares that if many people see and hear a gun fired, the further they are situated from it the longer is the interval between the seeing and the hearing. He thinks it is therefore "natural to suppose that the sound travels over the intervening space, in which case something must be happening even in places where there is no one with ears to hear."[25] But why does it seem "natural" to suppose this? Does not the "naturalness" receive its support from the experimental confirmations which are found for such assumptions in the context of our manipulating physical objects? Russell also thinks that, although the phenomenalist view (that there are no unperceived events) is not logically impossible, it is an unplausible view, because it is incompatible with physical determinism.[26] But why is the assumption unplausible that "imaginary" or "fictitious" entities are causally efficacious? If the unplausibility does not rest upon the findings of disciplined experience, embodied in common-sense knowledge, upon what can it rest?

Though Russell speaks much of "inferring" things, it is not clear in what sense he believes physical objects to be "inferred" from

25. *AM*, 209.
26. *AM*, 214.

perceptions. He uses the term "inference" in at least the following distinct ways: in the ordinary sense of logically deducing one proposition from another; in the familiar sense of asserting a proposition on evidence which makes that proposition probable; in the sense in which something which is perceived with an "accompanying interpretation" is obtained from something else that is supposed to be perceived directly or without interpretation, and finally, in the sense in which something that is a logical construction is obtained from entities out of which it is constructed. It is evident that when Russell says that the sun is inferred from our percepts, he does not mean that it is inferred in either of the first two senses specified, and he repeatedly asserts that he does not mean it in these senses. On the other hand, he declares that

> So long as naive realism remained tenable, perception was knowledge of a physical object, obtained through the senses, not by inference. But in accepting the causal theory of perception we have committed ourselves to the view that perception gives no immediate knowledge of a physical object, but at best a datum for inference.[27]

In this passage Russell is apparently using the third sense of "infernece"; and when he uses the term in this way he sometimes talks of an inference as an unconscious physiological process. But elsewhere he also says that "Modern physics reduces matter to a set of events. . . . The events that take the place of matter in the old sense are inferred from their effect on eyes, photographic plates, and other instruments. . . ."[28] And in this passage what is "inferred" is a physical object, viewed as a construction out of such events as perceptions. Russell does not therefore distinguish between the last two senses of "inference" listed above, and as a consequence it is difficult to extract a coherent formulation of how physical objects are inferred from percepts. However that may be, if our knowledge of the sun is "inferred" in the third sense of the term, the inference is presumably grounded in the causal theory of perception, and therefore in the procedures involved in common-sense knowledge of things. On the other hand, if that knowledge is "inferential" in

27. *AM*, 218. Cf. also: "Our knowledge of the physical world is not at first inferential, but this is only because we take our precepts to *be* the physical world." *P*, 130.

28. *P*, 157.

the fourth sense, the fact that the sun is a logical construction (if it is a fact) in no way prejudices the claim that we do have knowledge of it; for the exhibition of the sun as a construction out of events like perceptions obviously requires knowledge of the sun.

3.

1. It is a common error of Russell's critics to interpret his view that the physical world is a logical construction, as if he intended to deny that there are physical objects in the ordinary sense of this phrase. For this misunderstanding he is at least partly to blame. Thus he declares: "Common sense imagines that when it sees a table it sees a table. This is gross delusion."[29] Again, commenting on Dr. Johnson's refutation of Berkeley, he maintains that "If he had known that his foot never touched the stone, and that both were only complicated systems of wave-motions, he might have been less satisfied with his refutation."[30] And elsewhere he says that on the view he is recommending, "the 'pushiness' of matter disappears altogether. . . . 'Matter' is a convenient formula for describing what happens where it isn't."[31]

There are indeed several not always compatible tendencies struggling for mastery in Russell's use of his supreme maxim for philosophizing. One of them is that represented by the conception of experience according to which the objects of what is immediately "known" are in the brain; a second is the view that if something is a logical construction, it is *we* who have constructed it in time; another is stated by the conception that so long as some "indubitable set of objects" can be specified which will satisfy given formulae, then any object in that set may be substituted for the "inferred" object satisfying those formulae; and a fourth is the view that an object is a construction when it is *analyzable* into a structure of identifiable elements.

It has already been argued that the first of these tendencies is essentially irrelevant to (or at any rate, can be kept distinct from) the use of Russell's maxim. The second is often explicitly disavowed by Russell himself, though he often betrays his disavowal. But be-

29. *ABC*, 213.
30. *P*, 279.
31. *P*, 159.

fore examining the incidence of the remaining two tendencies upon his reconstruction of physical theory, I want to comment on the passages cited from Russell in the opening paragraph of this section. Is it a delusion when, under appropriate circumstances, we claim to see a table? A table may indeed be a logical construction; but in the sense in which we ordinarily use the words "see" and "table," it may be quite true that we do see a table. this mode of expressing what is happening is the appropriate way of putting the matter. Again, if when Dr. Johnson kicked a stone his foot never touched the stone, what *did* his foot do? To say that his foot never touched the stone, because both his foot and the stone were systems of radiation, is to *misuse* language, for in the specified context the words "foot," "stone," "kicked," and "touched" are being so used that it is correct to say Dr. Johnson kicked a stone and therefore his foot touched it. To be sure, under some other circumstances, and for the sake of certain ends, it might be advisable to use a different language in describing what had happened. But it obviously cannot be wrong to employ ordinary language in accordance with ordinary usage. And finally, it seems to me grotesque to say that the "pushiness" of matter can disappear as a consequence of a new analysis or redefinition of matter. We have learned to apply the word "pushy" to certain identifiable characteristics of material objects; and such a use of the word is correct, simply because that is the usage that had been established for it. Whatever may be the outcome of analyzing material objects, their identifiable properties will remain their identifiable properties, and it will be correct to apply the standardized expressions to them. It will certaintly not be correct to designate a physical body as a formula.

2. Let us turn to Russell's re-interpretation of physics. The first question I want to ask is what marks, if any, distinguish something which is a construction from something that is not. Russell seems to suggest at least two. One is the suggestion that something is a construction when it has properties which satisfy some mathematical formula or equation. He says, for example,

> The electron has very convenient properties, and is therefore probably a logical structure upon which we concentrate attention just because of these properties. A rather haphazard set of particulars may be capable of being collected into groups each of which has very agreeable smooth mathematical properties; but we have no right to suppose Nature

so kind to the mathematician as to have created particulars with just such properties as he would wish to find.[32]

One doesn't know how seriously to take such statements, especially since they imply, what is questionably the case, that it is we who invariably manufacture the properties which are convenient for the purposes of mathematical physics. It is certainly not evident what right we have to suppose that we have no right to suppose that Nature created at least some of them. It is one thing to say that for the sake of developing mathematical physics we have *isolated* certain features of things and ignored others; it is quite another thing to maintain that what we have selected we have also manufactured. Moreover, it is not clear why, on this criterion, the events out of which electrons and other objects are said to be constructions should not themselves be regarded as constructions. After all, as will be seen presently, they too have remarkably smooth mathematical properties: they fall into groups having exquisitely neat internal structures.

The second suggestion is more important. According to it, something is a construction when it is complex. Accordingly, since physical bodies as well as scientific objects like electrons are analyzable —indeed, on Russell's view into relations between ultimate simples— whereas perceptions and other events are not, the former are constructions out of the latter. The "ultimate furniture of the world" thus consists of a very large, perhaps infinite, number of events which have various specific relations to each other. When described in terms of spatio-temporal characteristics, these "particulars" are assumed to have quite small spatial and temporal dimensions. Moreover, some of these particulars (though not all) are perceived, and at least some of their qualities and relations are also immediately apprehended. Events, their simple qualities and their relations, are thus the building materials, the "crude data," in terms of which physics is to be "interpreted."

Russell admits that, although he believes his particulars are simples, in the sense that they have no "parts" or internal "structure," it is impossible to prove once for all that they are such. And although he also admits that simples are not directly experienced "but known only inferentially as the limit of analysis," he maintains it is desir-

32. *AM*, 319. At another place Russell proposes as a supplement to Occam's razor the principle "What is logically convenient to be artificial." *AM*, 290.

able to exhibit objects as constructions out of simples. His belief in the existence of simples rests on self-evidence: "It seems obvious to me . . . that what is complex must be composed of simples, though the number of constituents may be infinite."[33] Against such a view it is arguable that simplictiy is a relative and systemic notion, and that the justification for taking anything to be a simple rests on the clarification, the systematization, or the control of subject-matter which follows from a given mode of analysis. The issue is, however, not of great importance for the sequel. An issue of more serious concern is raised by Russell's admission that simples can be known only as the limits of analysis. For in the first place, he must also admit that we cannot in consequence literally *begin* with simples, trace through sequentially the complex patterns of their interrelations, and so finally reach the familiar objects of daily life. And in the second place, it becomes difficult to understand, even if we did succeed in exhibiting objects as constructions out of simples, just what such an analysis contributes to bridging the gulf between the propositions of physics and the familiar world of daily experience. However, Russell's subsequent analyses are not vitally affected by these doubts: whether events are ultimate particulars or not, the important part of his claim is that at least some of them are perceptions, and that they are relevant to the analysis only because of their relations to other things, and not because of a demonstrated lack of internal structure.[34]

One point is clear: Russell does not exhibit the logical structure of the physical world *entirely* in terms of entities which he regards as "known," since his particulars include events that are not perceptions. Such events are held by him to be "inferred," largely on the strength of the causal theory of perception and in order to avoid the "unplausible" consequences of a radical phenomenalism. Russell's own remark on the inclusion of unperceived (and therefore "inferred") events into the ultimate furniture of the world is one that many of his readers must have whispered to themselves: "If

33. *CBP*, 375.

34. Russell declares in this connection "Atoms were formerly particulars; now they have ceased to be so. But that has not falsified the chemical propositions that can be enunciated without taking account of their structure." *AM*, 278. The first sentence in this passage is seriously misleading, since it suggests that whether something is a particular or not depends on the state of our knowledge, and that therefore a construction is something made by us.

we have once admitted unperceived events, there is no very obvious reason for picking and choosing among the events which physics leads us to infer."[35] How many needless excursions into sterile epistemological speculations could have been avoided if this remark had been taken seriously! But the remark does make it plain that the significance of exhibiting things as constructions does not consist in circumventing the need for making inferences or in denying the existence of physical objects. The remark shows that the importance of the enterprise lies in *analyzing* or *defining* the sense of such expressions as "physical object," "point," "electron," and so on.

3. Russell's definition of physical object as a class of classes of events, related by certain laws of "perspective" and causal laws, is well known. It is unnecessary to dwell upon it here. It is sufficient to note that his analysis is motivated by the desire to show the otiose character of the traditional assumption of permanent, indestructible substances which mysteriously underlie the flux of events. I shall, however, examine his analysis of points (or point-instants), in order to suggest what seems to me a fundamental criticism of the approach to the "critique of abstractions" which Russell credits to, and shares with, Whitehead.

There is an obvious need for an analysis of points, if we are to become clear about the way in which the formulations of theoretical science are applied to matters of concrete experience. The term occurs in physical geometry, mathematical dynamics, and many other theories; and these theories are admittedly successful in organizing and predicting the course of events. At the same time, there seems to be nothing in our experience which corresponds to the term. The postulation of points as unique types of existences will not solve the problem, since such a postulation does not answer the question just how points are connected with the gross materials of experience. As Russell says, "What we know about points is that they are useful technically—so useful that we must seek an interpretation of the propositions in which, symbolically, they occur." His own answer to the problem consists in specifying certain "structures having certain geometrical properties and composed of the raw material of the physical world."[36]

35. *AM*, 325.
36. *AM*, 290, 294.

In outline, Russell's definition of point-instants is as follows: Every event is "compresent" with a number of others; i.e., every event has a common "region" with an indefinite number of other events, although the latter do not necessarily overlap with each other. If five events are compresent with each other, they are said to be related by the relation called "co-punctuality." If in a group of five or more events every set of five events has the relation of co-punctuality, the group is said to be co-punctual. And finally, if a co-punctual group cannot be enlarged without losing its co-punctual character, the group is called a "point." It only remains to show that points so defined exist; and to show this it is sufficient to assume that "all events (or at least all events co-punctual with a given co-punctual quintet) can be well-ordered"—an assumption that Russell proceeds to make.[37] And since it turns out that points thus specified satisfy all the usual mathematical requirements, Russell believes he has satisfactorily exhibited the logical construction of points.

Nevertheless, the analysis is to me very perplexing. Let me first call attention to an observation already made. Events, in the sense in which Russell uses the term, are the *termini* of analysis, and if they are apprehended by us at all they are not apprehended as psychological primitives. In this sense, therefore, events are not the "raw materials" of adult experience, whatever else might be the case for infants and other animals. If a point is what Russell defines it to be, the physicist who wishes to make a concrete application of statements about points must therefore first proceed to *isolate* the material (events) in terms of which points are to be eventually identified. In order to carry through this process, the physicist will certainly have to make use of the distinctions and findings of gross, macroscopic experience. But this is not all. Assuming that events have been isolated, co-punctual groups of events must next be found. However, since a co-punctual group may have an indefinite number of event-members, the assertion that a given group is co-punctual will in general be a *hypothesis.* The situation does not become easier when the physicist next tries to identify those co-punctual groups which are points: the assertion that a class of events is a point will be a conjecture for which only

37. *AM*, 299. A class is said to be well-ordered if its members can be serially arranged in such a way that every sub-class in this series has a first member.

the most incomplete sort of evidence can be available. If, as Russell believes, the existence of physical objects involves "inference," those inferences pale in comparison with the inferences required to assert the existence of points.

I now come to the serious basis of my perplexity. Russell's definition exhibits no concern whatever for the way in which physicists *actually use* expressions like "point." In the first place, it is certainly not evident that physicists do in fact apply the term to structures of events. On the contrary, there is some evidence to show that they employ it in a somewhat different fashion, using it in connection with bodies identifiable in gross experience and whose magnitudes vary from case to case according to the needs of specific problems. To be sure, the application of the term is frequently sloppy and vague, and its rules cannot in general be made precise. But the vagueness and sloppiness are facts which a philosophy of science must face squarely, and they cannot be circumvented by an ingenious but essentially irrelevant proposal as to how the term *might* be used.

This brings me to another phase of the difficulty It has already been noted that Russell does not always distinguish between two distinct views as to what is required in order to exhibit the logical structure of an object· on one of them, the logical construction of an object is exhibited when some "indubitable set of objects" is specified which satisfies a given formula; on the other view, the logical construction of an object is exhibited when statements about that object are so interpreted that the interpretations make explicit how those statements are *used* (or alternately, what those statements "mean"). The difference between these views is profound; and if the supreme maxim of philosophizing is to eventuate in clarification and not simply in a highly ingenious symbolic construction, it is the second view which must be adopted. Certainly Russell himself must have imagined himself to be acting upon this second view when he claimed that his account of the cardinal numbers made intelligible their application to the world of countable objects. On the other hand, his definition of points and other scientific objects conforms only to the requirements of the first view, and thereby offers no indication of the connection between the abstractions of physics and the familiar world. Like the definitions given by Whitehead with the aid of the principle of ex-

tensive abstraction, Russell's definitions formulate what are in effect another set of abstract formulae, quite out of touch with the accessible materials of the world. His "interpretation" of the equations of physics thus yields only another mathematical system, with respect to which the same problems that initiated the entire analysis emerge once more.[38]

4. One further set of general issues remains to be discussed. One of these issues arises in connection with Russell's redefinition of matter (common-sense objects, electrons, etc.) so as to avoid the hypothesis of an underlying permanent substance. He declares:

> The events out of which we have been constructing the physical world are very different from matter as traditionally understood. Matter was expected to be impenetrable and indestructible. The matter that we construct is impenetrable as a result of definition: the matter in a place is all the events that are there, and consequently no other event or piece of matter can be there. This is a tautology, not a physical fact. . . . Indestructibility, on the other hand, is an empirical property, believed to be approximately but not exactly possessed by matter. . . .[39]

And elsewhere he asserts that "Impenetrability used to be a noble property of matter, a kind of Declaration of Independence; now it is a merely tautological result of the way in which matter is defined."[40] Russell is of course right in calling attention to the fact that many propositions in physics as well as in everyday discourse are not contingent, since they are definitional in nature. It is not always clear which propositions have this character, and

38. One need only compare Russell's definitions of points with such analyses as those of Mach concerning mass and temperature or those of N. R. Campbell concerning physical measurement, to appreciate the difference between an analysis which is quasi-mathematics and an analysis which is directed toward actual usage.

It is also interesting to note that Russell criticizes one of Eddington's interpretations of certain equations in relativity theory in a spirit analogous to the criticism which the above paragraph makes of him. Eddington reads these equations to signify that electrons adjust their dimensions to the radius of curvature of the universe, and maintains that this adjustment can be ascertained by "direct measurement." Russell's remarks are as follows: "Now the electron may be, theoretically, a perfect spatial unit, but we certainly cannot compare its size with that of larger bodies *directly*, without assuming any previous physical knowledge. It seems that Prof. Eddington is postulating an ideal observer, who can see electrons just as directly as . . . we can see a metre rod. In short his 'direct measurement' is an operation as abstract and theoretical as his mathematical symbolism." *AM*, 92.

39. *AM*, 385.

40. *P*, 279. See also *ABC*, 185, and *CBP*, 366.

the difficulty in identifying them arises partly from the fact that the body of our knowledge can be organized in different ways. For example, if the equality in weight of two objects is defined in terms of their being in equilibrium when placed at the extremities of a lever which is supported at its mid-point, the law of the lever for this individual case is a truistic consequence of this mode of measuring weights. But if the principle of the lever is now a definition, the law of spring balances (Hooke's law) is not, and is empirically contingent. It is, however, possible to define the equality of weights in terms of Hooke's law, so that, although this law now functions as part of a definition, the law of the lever acquires the status of a contingent physical principle. Accordingly, to say that a law is a convention or tautology requires supplementation by a specification of its function in a particular systematization of physics, where the system of physics as a whole is not itself accepted on definitional grounds. It is thus not obvious that in every use of the words "impenetrability" and "matter," the impenetrability of matter is a logically necessary truth. For example, it is an empirical fact that a mixture of equal volumes of alcohol and water occupies a volume less than the arithmetical sum of the two, whereas a mixture of two equal volumes of water occupies a volume equal to this sum. If the concept of impenetrability is applied to this case, the impenetrability of matter appears to be a contingent truth. The point is, of course, that an "interpretation" of physics which leads to equating a logically necessary proposition with one that is contingent cannot be correct.[41]

41. Russell is often careless in some of his judgments as to which propositions are definitional. His definition of physical object leads him to say that "Things are those series of aspects which obey the laws of physics," (*OKEW*, 117), from which it would seem to follow that the laws of physics are definitions. Indeed, he does say that "Almost all the 'great principles' of traditional physics turn out to be like the 'great law' that there are always three feet to a yard," (*ABC*, 221). This is palpably absurd when taken without serious qualifications, and in this connection one must remind Russell of one of his own jibes against certain philosophers "Dr. Schiller says that the external world was first discovered by a low marine animal he calls 'Grumps,' who swallowed a bit of rock that disagreed with him, and argued that he would not have given himself such a pain, and therefore there must be an external world. One is tempted to think that, at the time when Professor Dewey wrote, many people in the newer countries had not yet made the disagreeable experience which Grumps made. Meanwhile, whatever accusation pragmatists may bring, I shall continue to protest that it was not I who made the world." In

A second general issue arises in connection with a technical detail in Russell's interpretation of physics. If the objects of theoretical physics are all constructions, then the symbols referring to them in the statements of physics are theoretically *eliminable*. Unfortunately, Russell has not formulated the translations of the requisite sort for specific statements which occur in treatises (e g., statements like "Zinc arsenite is insoluble in water"), although he has of course indicated the general procedure to be followed in constructing such translations. There are, however, fairly good reasons for doubting whether the elimination of symbols for constructs can be carried through without introducing assumptions of a dubious character. These reasons are based on the fact that in various parts of mathematics as well as in the empirical sciences certain expressions are usually so defined that in general they cannot be eliminated by the help of methods customarily accepted. For example, so-called "functor-expressions" like "the sum of," are often defined recursively, so that such expressions cannot be eliminated from statements like "The sum of x and y is equal to the sum of y and x." And if the so-called "dispositional predicates," like "soluble," are introduced into physics by the help of conditional definitions, as Carnap has suggested, an analogous difficulty arises with respect to them. To be sure, the desired elimination can be effected, provided we are willing to employ variables of a sufficiently high type; but the use of such variables appears to involve an "ontology" which it is not easy to accept. In particular, if we recall Russell's definition of classes and his view that a physical body is a class of classes of events, a statement about a body must finally be replaced by a statement about a property of properties—that is, about a property which is at least of type two. But does the assumption that there is such a property contribute much toward "assimilating" physics to the crude materials of perception? It seems to me, therefore, that, instead of making the elimination of symbols for constructs the goal of the logical analysis of physics,

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"Professor Dewey's 'Essays in Experimental Logic,'" *The Journal of Philosophy*, Vol. XVI (1919), 26. On the other hand, Russell himself recognizes the limitations in the view that physics is a huge tautology. In a penetrating brief critique of Eddington, he notes that the allegedly tautological character of the principles of the conservation of mass and of momentum holds only "in the deductive system [of physics]: in their empirical meanings these laws are by no means logical necessities." *AM*, 89.

a more reasonable and fruitful objective would be the following: to render explicit the pattern of interconnections between constructs and observations, on the strength of which these latter can function as relevant evidence for theories about the former.

I have been stressing throughout this essay the limitations of Russell's approach to the logical problems of science, and I have not thought it worthwhile to underscore their well-known excellencies. No student of his writings can fail to acknowledge the great service Russell's analyses have rendered to an adequate understanding of the mathematical sciences. He has made plain the highly selective character of physical theories, as well as the intricate transformations and reorganizations of sensory material which are involved in their use. He has exhibited the semi-arbitrary character of many symbolic constructions and the definitional nature of many physical propositions; and he has devised powerful techniques for isolating, and in some measure reducing, such arbitrariness and conventionality. Russell has not said the last word upon these matters; but he has certainly inspired a great multitude of students to try to say a better one. If the example of his own splendid devotion to independent thinking counts for anything, it is safe to believe that he would not prefer to have a different estimate placed upon his efforts.

11

Mr. Russell on Meaning and Truth

The ostensible aim of Mr. Russell's latest book[1]—the substance of his William James Lectures at Harvard—is to specify what is meant by "empirical evidence" and to determine what the connections are between such evidence and materially true propositions. But his long and repetitious discussion of these questions meanders over a large territory and touches upon most of the traditional problems in the theory of knowledge. In this review I wish to examine a few of the issues he raises.

I.

Mr. Russell's entire discussion is controlled by a fundamental distinction he draws between two types of inquiry frequently in-

1. *An Inquiry into Meaning and Truth.* Bertrand Russell. New York. W. W. Norton & Company, 1940. 445 pp. $3.75.

cluded in the theory of knowledge. The first aims to discover what sort of phenomenon knowledge is and how it is acquired, and it proceeds by studying the behavior of living organisms under appropriate circumstances. It therefore takes for granted the scientific account of the world and is an inquiry into matters of fact on par with any other scientific discipline.

The second type of inquiry does not take science for granted and is said to be philosophically more important than the first.

> We all start from "naive realism," i.e., the doctrine that things are what they seem. We think that grass is green, that stones are hard, and that snow is cold. But physics assures us that the greenness of grass, the hardness of stones, and the coldness of now, are not the greenness, hardness, and coldness that we know in our own experience, but something very different. The observer, when he seems to himself to be observing a stone, is really, if physics is to be believed, observing the effects of the stone upon himself. Thus science seems to be at war with itself: when it most means to be objective, it finds itself plunged into subjectivity against its will. Naive realism leads to physics, and physics, if true, shows that naive realism is false. Therefore naive realism, if true, is false; therefore it is false. (P. 14.)

It is therefore necessary to re-examine what passes for knowledge and discover the ultimate premises on which claims to knowledge must be based. These "epistemological premises" must possess the following three characteristics: they must be *logically prior* to other propositions, and thus serve as starting points for deduction; they must be *psychologically prior*, and so be beliefs which are not caused by other beliefs; and they must be *true* as far as we can ascertain. Mr. Russell believes that there are epistemological premises, and his inquiry is directed toward discovering their distinctive nature. In fact, he holds that if we are to escape from complete scepticism, we must assume "perceptive premises" as basic—beliefs which are "caused, as immediately as possible, by a percept" (p. 168), and whose truth is wholly dependent on the one occurrence we "notice" at the time (p. 172). What we notice, however, are percepts, and percepts are inside our heads (p. 428).

Mr. Russell is so convinced that the "common-sense" view of the world is false, that he nowhere argues the point. The basis for Mr. Russell's rejection of "naive realism" is presumably the fact that a complicated causal chain mediates between the occurrence of physical events and our perceptions of them, and that without

the existence of an appropriate physical and physiological mechanism there would be no perception of qualities. But it is by no means evident how it follows from this that the grass is not green in the familiar sense of the term, and that greenness is a quality located in a "perceptual space" distinct from the "physical space" occupied by the grass. Indeed, it is not *physics* which requires us to assume this, for physics is concerned with determining the conditions under which such qualities as greenness occur, and it identifies and locates such sensory qualities in much the same way as it identifies and locates electro-magnetic disturbances. It is Mr. Russell's *interpretation of physics* in terms of an epistemological dualism, rather than any special findings of the sciences, which require the exclusion of qualities from "physical space." The question is too involved to be settled in a review. But it is worth noting that Mr. Russell's interpretation of science is not the exclusive one, and that not all the alternatives lead to the radical bifurcation into "physical" and "perceptual spaces" which accompany his view. Considering the importance of the issue, it is therefore somewhat surprising that he does not even mention the views of objective-relativists like Professor McGilvary, which avoid such a bifurcation, or the view, advanced in 1936 by Mr. G. A. Paul and more recently by Mr. A. Ayer, that the "location" of perceived qualities in a distinctive "perceptual space" does not involve questions of fact but only of linguistic convention.

However that may be, there are a number of difficulties internal to Mr. Russell's fundamental distinction between the physical and the perceptual, and it will be instructive to consider a few of them. In discussing the relation between perception and knowledge he presently asks "What we want to know is the *resemblance*, if any, between the sun and 'seeing the sun'; for it is only in so far as there is a resemblance that the latter can be a source of knowledge concerning the former" (p. 146). This is a curious problem indeed; for if the sun is in physical space while the percept of the sun is in visual space, and since on Mr. Russell's view the organs of vision can yield only the percept of the sun, it would not seem rash to conclude that to compare them is *a priori* impossible. And yet Mr. Russell assures us that the sun looks round and also is round (p. 147), and entertains the possibility that the coördinates of physical stars in physical space are the same as the coördinates

of visual stars in visual space (p. 300). If the shapes or magnitudes seen by eyes occur in the same space in which eyes occur, the question whether two shapes are alike has a definite sense, even if technical difficulties may prevent a ready answer. But in what sense can we compare a sensory quality with something which, by hypothesis and not simply because of practical or technical difficulties, is inaccessible to the organs of sense?

Mr. Russell proceeds on the tacit assumption that physics and ordinary vision offer two *competing* conceptions of the world, with physics giving the true account. He maintains, however, that the world which physics is supposed to explore is simply a hypothesis which simplifies the statement of causal laws, but against which there can be no argument, "since experience will be the same whether it exists or not" (p. 294). How on this view of the "physical world" physics can refute "common-sense" is not apparent. On the other hand, Mr. Russell also says that while the space of physics is not directly sensible, it is nevertheless "definable by relation to sensible spaces" (p. 356), and this suggests that physical space is something that *might* be perceived if our "perceptive faculties were sufficiently extensive" (p 282). But in that case how can the world of physics be a hypothesis for which no critical evidence can be obtained? It would be easy to follow Mr. Russell if he maintained that physical space is not capable of being perceived, because "physical space" is simply the name for the order or structure of events and bodies—and is therefore not subject-matter for perception for exactly the same reason that the principles of statics are not subject-matter for perception. But he quite clearly does not mean this, for he declares that

> Unsophisticated common sense supposes that the book, just as it appears when seen, is there all the time. This we know to be false. The book which can exist unseen must, if it exists, be the sort of thing that physics says it is, which is quite unlike what we see. (P 293.)

It is difficult to see what sense it makes to say that while physical space is inaccessible to perception it is nevertheless definable in perceptual terms.

Again, Mr. Russell declares that such an experience as seeing a cat is veridical if there is an antecedent causal chain which, at a certain point in its backward course, reaches a cat (p. 150). But it is not clear to what question this is alleged to be an answer. Is

it an account of the physical conditions necessary for the occurrence of certain organic responses, or is it a criterion for the validity of perceptual propositions? It does not seem likely that it is the former, for the problem is not one for the theory of knowledge, and in any case Mr. Russell has too much respect for the requirements of natural science to regard his answer as solving it. But if it is the latter, how can Mr. Russell escape from a vicious infinite regress? For suppose I wish to determine whether my experience is verdical. I must then, on Mr. Russell's criterion, investigate the causal chain which leads backward from my percept in order to determine whether it contains a cat. But for this purpose I must use my sense-organs which, by hypothesis, yield only further percepts. I must therefore examine the causal chains leading back from these new percepts to see whether the appropriate causes occur in them, and so on *ad infinitum.* Mr. Russell's difficulties are somewhat reminiscent of the mathematician's well-known prescription for catching a lion: you must first carefully construct a circular corral with yourself inside and the lion outside; then perform a transformation by inversion, which brings the lion into the enclosure and takes you outside. Mr. Russell's problem is how to catch a cat: your percept is safely within the circle of visual space and the real cat is in physical space; but unlike the mathematician, Mr. Russell does not state what transformation will permit the two to move from one space to the other.

2.

In order to discuss his main problem Mr. Russell finds it necessary to construct first a "primary" or "object" language, whose initial vocabulary is to consist of names, predicates, and relation-words, all of which are to be ostensively defined. Sentences in this language are obtained by combining these words according to syntactical rules. But a sentence has both a subjective and an objective aspect: subjectively, it expresses a state of the user of the sentence, and its significance is what it expresses; objectively, if it is true it indicates a fact. Propositions are defined in terms of sentences: a proposition is the class of all sentences which have the same significance as a given sentence, although they are also defined as "psychological and physiological occurrences of certain sorts—complex images, expectations, etc." (p. 237).

A number of special assumptions control the construction of this primary language, and these must now be examined. The first is connected with Tarski's proof that if we are to avoid antinomies from the use of the words "true" and "false," we must admit a hierarchy of languages· a sentence in a given language can be characterized as true or false only by sentences belonging to a higher level of the hierarchy. Mr. Russell concludes from this result that there must be a language of the lowest level in which the terms "true" and "false" do not occur: "The hierarchy must extend upwards indefinitely, but not downwards, since, if it did, language could never get started. There must, therefore, be a language of lowest type" (p. 76).

It is not evident, however, that there must indeed be a language of the lowest type. Tarski's results do not preclude the possibility of a hierarchy of languages which is "open" at both ends; and they do not, it seems to me, even exclude the view that for historical languages the "hierarchy" simply requires that a distinction be made between statements according as they do or do not predicate truth of one another—a distinction which is relative to the context in which the terms "true" and "false" occur. Moreover, when Mr. Russell argues for the existence of a lowest-level language on the ground that otherwise language could not get started, does he mean "get started" in the historical development of languages or in the logical reconstruction of a language? If he means the former, is there any evidence that the historical languages have begun their development with an initial lowest-level language, or that at any time they consistently distinguish between statements of different levels? And if he means the latter, is it not perfectly evident that a logical reconstruction can be given for a language which contains the words "true" and "false," since a formalization of a semantic meta-language is possible without requiring the prior formalization of a language free from semantic terms? Mr. Russell's appeal to recent work in semantics to support his assumption of a lowest-level language seems to me entirely gratuitous.

A second principle controlling the construction of Mr. Russell's object language is his distinction between words which have a meaning only in suitable contexts (such as the word "than"): and words (such as proper names) which have a "meaning in isolation" and "which have been learnt without its being necessary to

have previously learnt any other words" (p. 80). Only the latter type of word is to occur in the primary language Since this distinction plays a fundamental rôle in Mr. Russell's book, it is regrettable that he does not discuss it more fully. How strictly is the phrase "in isolation" to be taken? Do any words have meaning in isolation from the various rules and linguistic habits which govern their combination with other words or their use in various contexts? On the other hand, Mr. Russell's view that there are words whose meanings may be learned solely by a "confrontation" with the objects they mean (p. 28) without depending upon their occurrences in sentences (p. 32), is dubious as a factual claim and question-begging when used to construct a theory of knowledge. Indeed, Mr. Russell himself admits that "the use of an object-word as *a complete exclamatory sentence* is its primary use, from which its use as part of a larger sentence is derivative" (p. 337, my italics). It is not easy to determine whether Mr. Russell is stating alleged matters of fact relevant to the psychology of learning, or whether he is arguing matters of logic. Taken as the former, he offers no respectable evidence on the matter; taken as the latter, he is misleading and unclear, if not definitely wrong, in assuming the existence of words having meanings "in isolation."

In any event, however, the sentences of the primary language are to be atomic in form and they are to express "judgments of perception": they will consist of a finite number of proper names and one word which is not a proper name but a predicate or a relation-word; and they will be the basic statements "credible independently of any argument in their favor" (p. 17). The *raison d'être* for the primary language is now evident; for "if due care is taken, all the sentences which embody empirical physical data will assert or deny propositions of atomic form" which will be "justified by observation without inference" (p. 53). Mr. Russell even suggests an explicit criterion for determining whether a word belongs to the primary language:

> Whenever you doubt or reject what you are told, your hearing does not belong to the object-language; for in such a case you are lingering on the words, whereas in the object-language the words are transparent, i.e., their effects upon your behavior depend only upon what they mean, and are, up to a point, identical with the effects that would result from the sensible presence of what they designate. (P. 84.)

A word is thus an object-word if you react "causally" to it. At the same time Mr. Russell recognizes the rôle which motor habits play in the use of words:

> . . . a black object may cause you to say "this is black" as a result of a mere mechanism, without any realization of the meaning of your words Indeed what is said in this thoughtless way is perhaps more likely to be true than what is said deliberately; for if you know English there is a causal connection between a black object and the word "black" . . . This is what gives such a high probability of truth to sentences stimulated by the presence of the objects to which they refer. (P. 72.)

It is, however, not altogether clear just what Mr. Russell thinks he can achieve through constructing his object-language. I shall waive discussing the factual claim that the occurrence of a black object can *cause* the utterance of certain words, since this is a matter for some branch of natural science to settle and it is a claim for which Mr. Russell produces no evidence whatsoever. I am frankly puzzled, however, by his appeal to habits for deciding whether an utterance is true. For the evaluation of habitual behavior requires just that sort of knowledge Mr. Russell is presumably putting to scrutiny, since habits are connected with familiar common-sense objects rather than with those brief, atomized events which are to be recorded in the primary language. Is there any reason why linguistic habits should be regarded as less reliable in connection with words applied to common-sense objects than when words are applied to atomic qualities—especially since the very apprehension of qualities as atomic is perhaps debatable? But I am no less puzzled by the relevance for Mr. Russell's problem of his claim that certain utterances are caused by certain occurrences; for if this claim is taken as a criterion for determining whether the utterances are true, an infinite regress seems unavoidable. Thus, to determine whether "this is black" is true, we should have to ask whether a black object causes it, and subsequently, in order to answer this question, whether other utterances made in the course of this process are true, and therefore whether certain objects cause *these* utterances, and so on indefinitely. In any case, we could not decide what words do belong to the object-language without first developing an adequate science which would discriminate and identify the causes of words, and we could not therefore construct a primary language without using those very assumptions which

the construction of the primary language is supposed to justify.

However that may be, there is one important corollary which must be noted concerning the character of Mr. Russell's object-language. He excludes from it all the familiar logical words such as "not," "or," "some," "all," etc., since these words according to him "presuppose the existence of language" and are needed only to express psychological attitudes and are not needed to indicate facts. Thus, "or" is taken to express experiences of hesitation and choice (p. 102), while "not" is related only to the act of judging (p. 89). This view requires some comments.

I can not help suspecting that Mr. Russell is badly confused in maintaining that logical words like "or" presuppose the existence of a language in the sense he intends. For it is surely not the case that these words are meta-linguistic ones, like "true" and "false," which presuppose a language in any other sense than the sense in which *any* word presupposes a language. For example, in the sentence "If I am hot then I perspire," the logical connective "if-then" does not stand between the *names* of two sentences, but is used to connect the two "facts" *denoted* by the two sentences "I am hot" and "I perspire"; and in this respect it functions like the two words "I" and "perspire" to convey information about matters of fact which are not linguistic or psychological. In *this* context, at any rate, the logical connective is used to *indicate*.

If logical words simply express psychological attitudes, how can Mr. Russell avoid psychologizing all of logic? Is the proposition "This is black or this is not black" a contingent truth, referring to facts of psychology? The triumph of *Principia Mathematica* over psychologizing logics would then have been a Pyrrhic victory indeed. On this question, however, Mr. Russell is regrettably silent. But is it the case that disjunctions *always* express simply psychological attitudes? Mr. Russell himself calls attention to such a term as "child" which is equivalent to "boy or girl," and which when used in sentences is normally employed to indicate and not merely to express; and other expressions in English, even color-names like "yellow," may be used in some contexts as abbreviations for disjunctions understood as logical sums rather than as expressions of hesitation. Since Mr. Russell insists that questions of psychology must be kept clear from questions of logic, it is not

clear why every usage of logical words must be excluded from the primary language.

Moreover, since a material conditional of the form "If *A* then *B*" is logically equivalent to a disjunction, Mr. Russell's primary language can contain no conditional statements either. He does maintain, however, that "the non-mental world can be completely described without the use of any logical words" (p. 114). Consequently, since words like "malleable," "mass," and other terms employed in the various sciences require to be defined with the help of conditionals—as independent evidence seems to show—the non-mental world can not, on Mr. Russell's view, contain the properties designated by such words, so that they must therefore be interpreted as expressing psychological facts. Is it not surprising how much Mr. Russell is able to know about the constitution of the physical world? The reader must, however, decide for himself whether he will place greater greater credence in the conclusions of Mr. Russell's epistemology, or in the conclusions of various sciences, resting on public evidence, that such properties do occur in the non-mental world.

It appears, therefore, that the price at which Mr. Russell's theory of knowledge must be bought is not only a psychologized logic but a psychologized physics as well. On his view propositions embodying the most certain and adequate knowledge must refer to absolutely determinate, absolutely specific atomic events; and the specificity of the evidence is for him not a function of the context in which evidence is required but is fixed antecedently for all contexts. It is perhaps not surprising that the only subject-matter he can find to which his evidential statements will refer is the subject-matter of individual psychology. And yet on one occasion Mr. Russell admits that "the difference between a statement which is disjunctive and one which is not does not consist in any difference in the state of affairs which would make it true, but solely in the question whether the difference between the possibilities which our statement leaves open is interesting to us or not" (p. 107). If this admission is taken seriously does it not suggest a clear alternative to his official view? Does it not raise the question whether the atomic character of statements is not, after all, relative to the type of problem requiring solution, that the degree of specificity which

the relevant evidence must satisfy is a function of the particular task at hand, and that the assumption of propositions which are atomic intrinsically is a gratuitous requirement of any actual inquiry?

3.

Mr. Russell's conception of perceptual judgments as caused by experiences requires him to assume that there exists immediate knowledge through the senses. This view is formulated by him repeatedly in many ways. Thus he declares that basic propositions must be known independently of inference and must be such that "the perceptive occurrence which gives the cause is considered to give the reason for believing the basic proposition" (p. 172). Again, if basic propositions are examined from "within" and not in terms of their causes, he maintains that

> every empirical proposition is based upon one or more sensible occurrences that were noticed when they occurred, or immediately after, while they still formed part of the specious present. Such occurrences, we shall say, are "known" when they are noticed. The word "know" has many meanings, and this is only one of them; but for the purpose of our inquiry it is fundamental. (P. 61.)

In this sense of "know," however, words are not involved, and Mr. Russell therefore asks how we can "formulate a sentence which (in a different sense) we 'know' to be true in virtue of the occurrence" (p. 61). Mr. Russell examines the question whether in *using* a language we do not, after all, rely on the effects of repetition and assume a certain universality:

> Repetition and universality, *in fact*, are of the essence of the matter, for language consists of habits, habit involves repetition, and repetition can only be of universals. But *in knowledge* none of this is necessary, since we use language, and can use it correctly, without being aware of the process by which we acquired it. (P. 393.)

The fundamental issue and its resolution therefore is:

> Can anything be learnt, and if so what, from a single experience? . . . Unless each single observation yields *some* knowledge, how can a succession of observations yield knowledge? (P. 395.)

The question as to whether there is "immediate knowledge" is always in danger of becoming a terminological one, and I do not wish to discuss it in this form. I shall ask instead whether the

problems raised by Mr. Russell in this context are resolved by him, and whether he has adequately established the existence of immediate knowledge in his sense of the term.

Mr. Russell's discussion of the question how we can formulate a sentence which we "know" to be true in virtue of the occurrence which we notice (p. 61 ff), seems to me to yield only a truistic answer. For in the end Mr. Russell tells us nothing more than that the sentence "I am hot," for example, expresses what I notice—namely, that I am hot. On the other hand, his view that the perceptive occurrence which is the cause of a basic proposition also gives the reason for believing it to be true, is a perfectly futile criterion for determining whether a proposition *is* true. Does Mr. Russell believe that we can ascertain by immediate inspection what the causes of our utterances are? This seems to be a large factual claim indeed, and it would be interesting to know what the controlled experiments are which establish it. But if he does not believe this, must we first determine by independent means what the causes of our beliefs are before we can say whether they are true? In that case, however, I do not see how we can avoid an obvious infinite regress. The tacit assumption underlying Mr. Russell's discussion is that we can be more certain of a proposition the more directly it is connected by causal relations with what we claim to know; but I do not know any good reason why this should be so.

Mr. Russell declares that only such propositions ought to be regarded as basic which are "about particular occurrences which, after a critical scrutiny, we still believe independently of any extraneous evidence in their favor" (p. 187). Such a scrutiny involves restriction of attention to the "momentary perception as the least questionable thing in our experience" (p. 189); nevertheless, "in order to study momentary visual space, it is necessary to keep the *eyes motionless* and attend to things near the periphery as well as the center of the field of vision" (p. 433, my italics). I fail to see, however, that if such operations must be performed in order to obtain basic propositions, how Mr. Russell can avoid using assumptions and cognitive commitments which are not directly presented as true in a momentary perception, assumptions in terms of which his scrutiny is made. Surely we can not control our eyes without taking for granted common-sense objects and thus assuming the

validity of inductive generalizations and inferences. Surely we can not scrutinize without analyzing, as Mr. Russell himself admits (p. 422), and we can not analyze without regarding some things as relevant and others as not, and thereby falling back upon general principles which are not established in a single momentary experience.

The type of argument Mr. Russell offers, to show that a single observation must yield some knowledge, could equally well establish the conclusion that every constituent note in a melody must itself be a melody if there is to be any melody at all; and if Mr. Russell were not writing epistemology, he would dismiss it as an instance of the fallacy of division. But a more serious difficulty is that on Mr. Russell's view there is no room whatsoever for the criticism of truth-claims in the case of perceptual judgments, since the distinction between truth and falsity in this case is "to be defined by their *causes*" rather than by their effects (p. 410). The ultimate factual premises are therefore such that no one of them can be rendered in any degree probable or improbable by any number of others, the sole reason for believing them being always the events which cause them. It is therefore not altogether surprising to find Mr. Russell abandoning "empiricism," as he conceives it, as an untenable standpoint, and being driven to accept a number of non-demonstrative principles of inference which experience can neither confirm nor refute, but which are nevertheless in some cases to be regarded as more certain than the evidence of the senses (p. 398). I believe, however, that the source of Mr. Russell's difficulties is his taking the distinction between what is prior and what posterior in knowledge as fixed once for all. He does not entertain the possibility that what is prior in one inquiry may be posterior in another context, and that the self-corrective procedure of inquiry shuttles systematically between general principles and observations, correcting each in the light of the other. On the contrary, as he himself explains, his analysis is controlled by the method of Cartesian doubt and by the desire to find a fixed set of propositions which would constitute the ultimate foundation for all other beliefs. This is indeed the conception of "linear justification" evident in the Aristotelian theory of science, in which all propositions hang like links in a chain from an unshakable first link. Since Mr. Russell regards Aristotle as one of the four great calamities in the history

of philosophy, he seems to be in very bad company indeed. This is not the place to develop or even to state adequately an alternative conception of "justification" for beliefs; but I submit that the beliefs held on the basis of modern scientific inquiry are neither acquired nor organized nor justified in the manner Mr. Russell thinks it is essential for accredited knowledge to be.

There is one final difficulty in Mr. Russell's reconstruction of knowledge. He explicitly declares that perhaps no actual propositions quite fulfill the conditions required for basic propositions, although he maintains that "pure perceptive propositions remain a limit to which we can approach indefinitely, and the nearer we approach them the smaller the risk of error" (p. 190). But this admission seems to me to jeopardize seriously his entire undertaking. For Mr. Russell constructed his primary language and postulated basic propositions in order to establish with their help the linkage between the facts of the non-verbal, non-mental world and the propositions accepted on the basis of crude experience or scientific inquiry. To the extent, however, that the existence of such basic propositions is not established, and if, moreover, the evidence available from independent sources is accepted as showing that no such propositions exist, Mr. Russell has failed in the task he had set for himself. For in that case he has simply constructed an abstract linguistic system the application of which to matters of fact raises precisely the same sort of logical issues as are germane to the use of the language of every-day discourse and science. This point, which I regard as quite central in this matter, may be made plainer by another illustration. Let us suppose it is a problem to show how a system of mathematics, developed on the assumption of a continuous field of variation, can be used to formulate the facts in a world of discrete objects; then the problem is clearly not solved if we construct another system of mathematics, also depending on assumptions of continuity, which is to mediate between the first system and the specified subject-matter. As I see it, Mr. Russell's epistemological construction likewise aims to mediate between the facts of the world and scientific propositions; but the assumptions he finds it necessary to make raise the very questions which that construction was intended to solve.

4.

With the help of his specially constructed object-language Mr. Russell believes he can resolve a number of issues which have plagued the traditional theory of knowledge and metaphysics. There is space to consider only some of his findings.

1. He insists that statements like "this is red" must not be construed as subject-predicate propositions; for if they are so understood "this" would be a name for an unknowable substance. Since he holds that "what would commonly be called a 'thing' is nothing but a bundle of co-existing qualities such as redness, hardness, etc.," he construes the above statement as being of the form "redness is here" (p. 120). Nevertheless, nowhere in the present book does Mr. Russell show in a general way how things are to be analyzed in the indicated manner—unless one regards the use of the metaphor "bundle" as performing this task. The difficulties in the way of giving such an analysis on a phenomenalistic basis are well known, and I believe it is correct to say that no satisfactory analysis is known which makes possible the elimination of thing-names from every context. The difficulties are certainly not diminished if with Mr. Russell we exclude logical words from the primary language. How can sentences containing thing-names be replaced by equivalent sentences containing only object-words, if the relations between qualities (such as are usually formulated by conditional sentences) in terms of which "things" may conceivably be defined are after all not relations between those qualities?

The analysis of things as bundles of qualities requires Mr. Russell to give a definition of "place." This he does by associating a pair of angular coördinates with each specific quality as it occurs in perceptual space, where the angular coördinates are themselves to be regarded as qualities (p. 122). It is not clear, however, in what sense angular coördinates are directly apprehended qualities; for the coördinates are presumably some type of *number* (if not, in what sense are they coördinates?), and at the very minimum they will have to be introduced by stipulating some order among qualities. Can such an ordering be performed in the momentary specious present? And if not, can angular coördinates be specified without using those very "common-sense" objects which it is the task of Mr. Russell to analyze into bundles of qualities? He admits that

the angular coordinates of latitude and longitude in "physical space" are not directly observed qualities, but insists nevertheless that "they are definable in terms of qualities" so that it is "a harmless avoidance of circumlocution to call them qualities" (p. 124). It is not clear what Mr. Russell means by "define"; but if he means that "latitude" and "longitude" can be replaced in every context by names of directly experienced qualities, his statement can be accepted only on faith, since he gives no clue as to how this is to be done.

2. Mr. Russell has much to say on how sentences indicate, and is particularly concerned with the question "How can I think of things that I do not experience?" (p. 274). His views briefly stated are somewhat as follows: When the verbal expression of a belief involves no apparent variables, what is expressed and what is indicated are identical. But when a sentence "involves" a variable, its relation to what it indicates is remote and indirect, and it refers to something beyond what is directly experienced. What is indicated by such a sentence, however, can not be stated in the primary language, since we do not have a proper name for something outside experience (p. 278). Thus, when I say "this dog is ten years old," what I experience is a canoid patch of color, and I am making a statement about an unexperienced something (the physical dog) which stands in a certain relation to the canoid patch of color and is also ten years old. Hence the sentence "this dog is ten years old" turns out to "involve" an apparent variable, since it must be rendered as "there is a *c* such that it is identical with the cause of the canoid patch of color and is ten years old." If we had a proper name, say "*A*," for the unexperienced something—which of course is impossible—this existential sentence would indicate that which makes "*A* is ten years old" true (p. 279). Mr. Russell also seems to think that this analysis shows how we can think of things we do not experience (p. 282).

Mr. Russell's argument is difficult to follow, and I am not clear as to what he thinks he has established. Taken quite strictly, the sentence "this dog is ten years old" surely does not *contain* an apparent variable, and the issue then is just what Mr. Russell means when he says that it nevertheless "involves" one. He may simply mean that a sentence of the form "There is an *x* such that *x* is ten years old" follows from the sentence "This dog is ten years

old," which surely is the case for many languages though not necessarily for all; but this fact can not be used to prove that the thing said to exist does not inhabit the same realm as the canoid patch of color. Such a consequence would be valid only if we antecedently accept Mr. Russell's theory of perceptual and physical spaces. Moreover, the above two sentences are not logically equivalent, for the second does not follow from the first; and yet Mr. Russell gives the impression that the first sentence formulates an analysis of what is said by the second. Nor do I see how Mr. Russell has answered his question about how we can think of things we do not experience in any way other than trivially: for all I find him saying is that we simply *can* think of things that are not being experienced. By transforming a sentence without variables into a sentence with variables, the relation of the latter to what it indicates is no less a problem (if it is a problem) than the relation of the former to what it formulates.

3. Mr. Russell discusses the nature of truth and its relation to knowledge, and after disposing of alternatives accepts a correspondence theory of truth. He distinguishes between an epistemological and a logical version of the correspondence theory. According to the former, the correspondence is between a proposition and an experience; according to the latter it is between a proposition and a fact. The important difference between these two versions is that while on the former a proposition is neither true nor false if there is no evidence for it, on the latter every proposition is either true or false, so that there must be a fact, whether it is observed or not, corresponding to one member of every pair of contradictory propositions (p. 364). More specifically, on the epistemological theory a true basic sentence corresponds to an experience or expresses an experience, where "corresponds" and "expresses" are to be defined behavioristically. Sentences which are not basic are "probable" if they stand in certain (unspecified) syntactical relations to basic sentences (p. 366). On the logical theory, a sentence is verifiable (and thus presumably corresponds to a fact) if it is either epistemologically basic or stands in certain (unspecified) syntactical relations to such basic sentences. However, on this theory unverifiable sentences will be true if they correspond (in some unspecified way) with facts, where "fact" is taken as an undefined term. Thus, if

we call those sentences "data" which express and indicate experienced facts, verifiable sentences will be those which are so related syntactically to data as to make them deducible from data. Hence true sentences are those which either indicate facts or have the same syntactical relations to sentences indicating facts which verifiable sentences have to data (p. 367). Since Mr. Russell finally accepts the logical version of the correspondence theory, he must maintain that verifiable sentences are a proper sub-class of true sentences and that a "proposition may be true although no method exists for discovering that it is so" (p. 361).

However, in spite of his long discussion of the correspondence theory, no clear view emerges as to what this correspondence is. Mr. Russell assures us that the correspondence between basic propositions and facts must be specified causally; but we are left in essential obscurity as to the nature of the correspondence in the case of propositions involving variables. He has nothing to say concerning the syntactical relations in virtue of which a proposition is said to be "probable" relative to basic propositions; and he does not even suggest what bearing "probability" would have upon the problems of inductive procedures if the term were defined on the basis of syntax alone. He maintains that by retaining the principle of excluded middle he requires the "metaphysical assumption" that there are "facts" corresponding either to a sentence or to its negative; but since "facts" are introduced to save a principle and are not otherwise identified, and since on his own analysis this principle does not describe anything in the non-verbal and non-mental world (for it contains logical words), it is something of a mystery how such a procedure can yield metaphysical conclusions. It is possible, and I think for many purposes highly desirable, to stipulate such conventions for the use of the term "truth" that as a consequence a proposition may be true even if "no method exists of discovering that it is so." On the other hand, many empiricists who take their point of departure from the procedures of science are ready to admit that for such a sense of "truth" there is no general method for discovering whether a proposition is true. But they would insist, and to my mind rightly so, that while this sense of truth may specify the ideal goal of inquiry and thus serve as a norm to it, it is not directly relevant for the admittedly different issue as to how

reliable beliefs are to be obtained or as to whether the methods of bringing evidence to bear upon a proposition are integral to an understanding of what the proposition asserts.

4. Mr. Russell's final problem is whether anything, and if so what, can be inferred from the structure of language as to the structure of the world. But I think he compromises his problem from the very start by insisting that since common-day language is not sufficiently precise the question can be raised only for an "artificial logical language" (p. 415). For how is this logical language to be constructed? Does it have to be constructed on the basis of a prior study of what does exist in the non-verbal world? If it does, what is the point of constructing such a language, since this prior study will then determine just what the structure of the language must be? In this case we would hardly regard as valuable any "inference" we might make about the structure of the world from the structure of the language. But if such a prior study does not have to be undertaken, how can the question be decided except on the basis of an antecedent—and therefore arbitrary—theory as to what exists?

However that may be, Mr. Russell maintains that there is a "discoverable relation between the structure of sentences and the structures of the occurrences to which the sentences refer" (p. 429). Here are some of his findings: If a proposition contains no variables, it can not have more than one verifier; if a number of subject-predicate statements expressing judgments of perception have the same subject, they will have the same verifier, namely, what is designated by the subject; if a number of such propositions have the same predicate, the verifiers all have a "common part," namely, what the predicate designates (p. 431). These results are disappointing, to say the least. They appear to be simply the analytical consequences of the semantical rules governing the construction of the sentences in question: that a sentence without variables, for example, has only one verifier, is just what was stipulated as to the circumstances under which a sentence is to be viewed as involving a variable. And on the crucial question whether from the structure of *one* sentence (however "structure" may be defined) we can infer anything about the structure of *its* verifier, Mr. Russell is disappointingly silent.

However, Mr. Russell does maintain that there seems to be no escape from admitting relations as parts of the non-linguistic constitution of the world, since, for example, *similarity* can not be explained away as belonging only to speech (p. 434). I fail to see, however, how the study of language *alone* (and in Mr. Russell's case it is the study of just one artificial language) can yield this conclusion. Have we proved that a certain relation occurs in the world because we are unable to eliminate a certain relation-word from a given language? Does not such a proof require the submission of relevant empirical or factual evidence? I strongly suspect that Mr. Russell can produce a non-verbal rabbit from his verbal hat only because he has taken care to bring the animal into the room.

In any event, Mr. Russell rejects a complete metaphysical scepticism, and maintains that "partly by means of the study of syntax, we can arrive at considerable knowledge concerning the structure of the world" (p. 438). In one very obvious sense this is surely the case: few will disagree with him if his thesis is that "by means of syntax" we can establish relations of deducibility between statements and thus make it possible to learn something about the structure of the world by examining the derived conclusions in the light of empirical evidence. But so interpreted Mr. Russell's claim can hardly be "the goal" of his long book. On the other hand, whether his claim is valid in any other sense than this one is difficult to say; for he has not formulated a single proposition about the structure of the world which he has unmistakably obtained from the study of the structure of language alone.

Mr. Russell's book suggests to me that the phoenix is perhaps not a mythological bird after all, and that its secret name is "epistemology." For although the type of inquiry Mr. Russell pursues has often been pronounced dead by eminent authorities, it manages to get itself born anew with unfailing regularity. It is true that in Mr. Russell's book it comes to life again adorned with strange plumage borrowed from current logical terminology; but it is unmistakably the same old bird and carries on in familiar ways. When so many excellent hands have failed in curtailing the charmed life of the creature, it is reasonable to resign oneself to its continued existence. In this review, however, I have tried to show

that Mr. Russell's book contributes little if anything to our understanding of the operations of language, of the conditions of meaningful discourse, or of the actual procedures involved in validating claims to knowledge. And if I have understood his book aright, it seems highly probable to me that the phoenix is not the bird of Minerva.

12

The Basis of Human Knowledge

SHORTLY BEFORE his departure from this country in 1944, Bertrand Russell expressed his intention to round out his career in philosophy with what he hoped would be his most important book. He declared that while most of his life had been devoted to the foundations of mathematics and the analysis of deductive inference, these subjects were in his opinion only of secondary significance. The paramount problems of philosophy, he believed, are concerned with the clarification of "non-demonstrative" arguments, whose premises imply their conclusions only with some degree of probability rather than with rigorous necessity. It is arguments of this type that play commanding rôles in the affairs of life as well as in controlled scientific inquiry; and the great "scandal of philosophy" is often said to consist in the fact that the principles underlying them are still obscure and debatable. To make explicit these principles, and to exhibit the assumptions required to establish their validity, were the problems to which Russell planned to address his next book.

Human Knowledge[1] is the realization of Russell's aim. It contains the fullest account he has ever given of his views on probable inference, and on the justification of knowledge that is claimed to be objective. But the book contains more than this. Russell was somewhat less than accurate in characterizing his long career in philosophy as having been concerned mainly with mathematical logic. On the contrary, he has been intensely preoccupied with central issues in epistemology for almost two score years. A by no means negligible value of the present book lies in its presentation, in systematic and perhaps final form, of analyses of special questions in theory of knowledge with which Russell has long been identified—questions ranging from problems of language and meaning, to issues connected with space, time, and causality. There have been shifts in the details of his philosophy both in emphasis and substance. Perhaps the most important of these, especially in reference to the argument of the present volume, is his *qualified* acceptance of what he used to recommend as the "supreme maxim of scientific philosophizing." According to this maxim, since inferences to unobservable objects are always precarious, they ought to be replaced by the entirely safe device of *defining* such supposed objects in terms of directly experienced data. Russell now believes that science cannot dispense with such inferences and has abandoned his earlier phenomenalism. Nevertheless, Russell's present account of the nature of knowledge does not differ essentially from the doctrines to which his readers have become accustomed. And in any case, the central problem of the book—the justification of scientific knowledge—is a direct consequence of the approach to the theory of knowledge he has been developing for several decades.

The logical starting-point of Russell's discussion is the assumption that if anything is to be known by inference, some things must be known directly and without inference. But for him what we know directly is inherently mental, private, and subjective; and his task is to show how, from the fragmentary data of sensation, genuine knowledge of the objective world can be obtained. Moreover, while the evidence for science comes from commonsense—the view according to which the objects of our experience

1. *Human Knowledge: Its Scope and Limits.* By Bertrand Russell. New York: Simon and Schuster, 1948. $5.00.

really do possess the qualities they appear to possess—Russell maintains that science proves common-sense to be false: roses are not really red, nor is the sun really hot and bright. What, then, is the warrantable content of scientific propositions, and how can we warrant them as valid?

Now it is evident that the conclusions of science cannot be deduced by strict logic from the data of direct experience; and if science is to be possible, various principles of non-demonstrative inference must therefore be accepted. However, these principles enable us to obtain objective knowledge only of abstract structures. The objective qualities of things—if indeed they have any—are therefore hidden from us. Furthermore, Russell maintains that these principles of inference are warranted only on condition that the world possesses certain general characteristics; and the climax of his analysis consists in enumerating and specifying the assumptions which, if true, are sufficient to justify scientific inference. On the other hand, these ultimate postulates of science cannot in turn be justified with the help of the usual methods of experimental science, since according to Russell scientific method can be shown to be valid only if we grant the truth of these postulates. Experience can confirm these ultimate assumptions, but it can neither prove them nor make them probable; and yet without knowing them to be true we can have no rational basis for our belief in science. Russell is thus driven to the conclusion that the postulates of scientific inference must be known in some extra-scientific fashion, so that empiricism as a theory of knowledge is found in the end to be inadequate. "Either we know something independently of experience, or science is moonshine."

As always, Russell develops his arguments with refreshing vigor and bold but responsible ingenuity, and his detailed discussion of special issues will undoubtedly revitalize professional thinking on these subjects. Nevertheless, at least two major difficulties will prevent some of his readers from accepting his main argument. In the first place, Russell assumes, in the company of a long line of distinguished philosophers, that all inferential knowledge must be based on knowledge that is both immediate and private. However, a careful examination of his examples of direct knowledge raises fundamental doubts as to whether anything is known in this way. For his examples, as well as his over-all analysis,

do not settle the question whether the alleged data of direct apprehension are *logically primitive*—as Russell's theory requires them to be—or whether the cognitive apprehension of those data is not actually contingent upon prior inferential processes and upon acquired modes of organic behavior. It is certainly not a matter of direct knowledge whether some specific item (for example, my seeing a flash of lightning) is known immediately or only by inference; and a considerate body of objective knowledge must be assumed when one attempts to establish whether a given datum of experience is logically primitive or involves inference. There is therefore ground for suspicion that the alleged necessity for basing all inferential knowledge on knowledge that is inherently private and subjective, is the dialectical necessity generated by Russell's framework of analysis, rather than the inescapable outcome of an independent inquiry into the circumstances under which knowledge is obtained. But if this is so, the relevance of much of his reconstruction of scientific knowledge is placed in serious jeopardy.

The second difficulty to which reference has been made is connected with Russell's central problem of justifying non-demonstrative inferences. Everyone will agree that we do not possess demonstrative knowledge of many things (for example, that the earth is a flattened sphere) which is nevertheless beyond reasonable doubt. In such cases, the evidence for what we claim to know is not sufficient for logically deducing the asserted conclusion, though it *is* sufficient for accepting the conclusion without serious risk of error. Now what is one to understand by the demand that such knowledge be justified? If the demand is so conceived that the conclusions of non-demonstrative inferences must be shown to be *logically necessary* consequences of premises known to be true, then it is obvious that no claim to objective knowledge can ever be justified—unless assumptions are introduced which must be known independently of experience. However, the impossibility of satisfying the demand without adopting assumptions of this sort does not prove that we can have no warranted knowledge except by abandoning empiricism. The impossibility simply calls attention to the fact that the demand is an inappropriate one. Why *should* we accept for non-demonstrative arguments standards of validity rightly required for strictly deductive ones? Now, indeed, Russell formally disavows the idea of seeking a deductive justifica-

tion for scientific inference. Nevertheless, his actual analysis is based squarely on a conception of "justification" that coincides with the one he nominally rejects. For he introduces postulates of scientific inference which must be known independently of experience, precisely because he wants to prove *deductively* that non-demonstrative inferences are valid. But if this idea is surrendered as hopeless because misguided, the fact that science employs such postulates as Russell mentions no longer requires to be interpreted as showing the limits of empiricism. Indeed, in adopting that interpretation Russell exhibits himself as operating, *malgré lui*, from the intellectual premises of traditional rationalism. On the contrary, those postulates can be intelligibly conceived as rules warranted by their matter-of-fact success in ordering experience. They are known to be true, not independently of experience, but on the basis of the same sort of empirical evidence that serves as the foundation for warranted knowledge throughout science.

Russell has not settled the question he proposed to resolve in this book. None the less, disagreement with his analyses—even disagreement with his major thesis—constitutes no bar to profound admiration for Russell's devotion to philosophy as a rational discipline, and for the scope and penetration of his discussion. "Human Knowledge" is an impressive and stimulating intellectual adventure, written in masterly prose by a master of contemporary thought.

13

Eddington's Philosophy of Physical Science

I.

THE BREAK WITH the familiar qualities of our everyday world, which modern science since Kepler and Galileo has introduced, has been accentuated by the recent developments in physics. Professor Eddington in this book[1] gives an account for the lay reader of the most striking of these developments, and argues for a reinterpretation of the place of the world of science and the world of daily experience in the scheme of reality. His conclusion is that the subject matter of physics is only a partial aspect of something wider, that mind, endowed with "freedom," is necessary to mediate between the realm of physical concepts and "actuality,"

1. *The Nature of the Physical World*, by A. S. Eddington. (Macmillan, 1928.) $3.75.

and that the intrinsic nature of this actuality is spiritual and akin to consciousness.

Eddington's philosophic kit is loaded with ideas that seem to be inherited from a darker philosophic age, and important use is made of them with no effort at their critical evaluation. It is a dogma with Eddington that the qualities we commonly associate with things are a mental product, that our daily experience is an illusion (p. xii). It is a dogma with him that knowledge is never of things external to us, but of certain associated qualities occurring in the brain, and that the steps of the inference to the external world exactly reverse the steps of the physical transmission which brought the information (pp. 270, 278). From these premises it follows that the world of physics is the "real" world, although it is only the structure of the real world that science obtains, since the intrinsic quality of things (with a solitary exception in the case of our own ego) is an inscrutable somewhat not known to us (p. 254).

It is for such reasons that Eddington finds the world of physics symbolical or abstract—symbolical, indeed, not of the things of daily experience (which have the status of appearance), but of the inner essences of this appearance. It is plausible to believe, however, that had he kept clear the distinction between *objects* of knowledge and the physical *conditions* or *correlates* of knowledge, he would have never arrived at this metaphysical dualism.

To the confounding of this distinction Eddington adds some others. Symbols have a reference beyond themselves, and at the same time symbols have specific characters of their own. Now there seems no reason in calling the world of physics symbolic if we are never permitted to know what the symbols stand for. The moves in a chess game may be symbolic of army manoeuvres, and the numerals in a book may be symbolic of counting; but if we never know what the moves in chess or the numerals stand for, it is not as *symbols* that they are studied since their referents do not determine the course of the inquiry. Either the referents of the world of physics do condition the symbolism, and then we know what they are, or physics is not symbolic. However, "knowledge" is a word with many meanings for Eddington: sometimes it means the possession or enjoyment of sensory and affective qualities; sometimes it means the possession of the representative or sign

function of qualities which are then not always "known" in the first sense. If one assumes the dogma that only the immediate is real, and if one takes knowledge in the second sense, it is easy, therefore, to argue that the symbolic character of knowledge makes it incapable of grasping reality. Nevertheless, it seems permissible to retort that knowledge, when it is taken as the inferential function, is symbolic as well as adequate to grasp reality: the psychophysical carriers of meanings are not like the meanings they carry, nor are the meanings like things; but the meanings are meanings *of* things and symbolize things in the sense that they denote types of experimentally obtainable reactions between things.

Eddington's difficulties with these matters come to a head in his treatment of change of entropy as symbolic of the irreversibility of time (pp. 88 ff.). Color for him is a quality purely mental, not an ingredient of the real world of electro-magnetic waves. But the dynamic character of time, of "becoming," he insists is a genuine quality of nature, even though the physical conditions for the recognition of this quality seem to be the same as those for the recognition of any other. To save "becoming" for nature, Eddington, consistently enough, must suppose that in this case the nerve mechanism does not intervene and that the dynamical character of time, grown suspiciously substantial for a relativist (cf. p. 97, where we are told of the "moving on" of time), is observed not through the sense organs but by a private door of the mind. Mind turns out to be something with physical properties dwelling in the body (cf. p. 311). Ordinarily there is a double symbolism in Eddington's system. Color is something that occurs in the mind, but is symbolic or equivalent in consciousness to the electro-magnetic waves. Secondly, these waves have a structure which is known but which is nevertheless symbolic of qualities not "known" and unknowable. If in the case of "becoming" this dualism vanishes it is because "I grasp the notion of becoming because I myself become. It is the innermost Ego of all which *is* and *becomes*" (p. 97).

In spite of Eddington's clear perception that nature is something richer than physics officially declares, he persists in confounding the subject matter of physics with the data of the physicist. His argument is that since all of our scientific information is summed up in measures, and since the measure-*numbers*, as such, offer no ground for making qualitative distinctions in nature, no such dis-

tinctions exist for science. But while it is true that at a certain level of investigation all that the scientist may use are the measure-numbers and be indifferent as to how they are obtained, surely a philosophy of science cannot stop at that level (if indeed it were even possible for science to do so). It is only when the locus of those measure-numbers is discovered in certain qualitative continuities, familiar enough in our common experience, that the predictive power of science becomes intelligible. It is adopting a very narrow behaviorism to say that "two tons *is* the reading of the pointer when the elephant is placed on a weighing machine," since an adequate statement of what a pointer reading is involves just those qualities which had been supposed excluded.

Eddington is bent upon showing that the characteristic features of the world of physics are introduced by Mind (with a capital): "ultimately it is the mind that decides what is lumber—what part of our building will shadow the things of common experience, and which has no such counterpart" (p. 235). However, his failure to indicate in what contexts such selections are made makes his account very unilluminating. In one sense, everything is decided by mind, since deciding is something that only a mind can do. But the force of Eddington's insistence upon the truistic character of physical laws is gone if one remembers that decision and choice can arise only when subject matter possesses traits which may be chosen. To say that the prominence of conservation laws is due to the mind's demand for permanence is doubtless important; some things are conserved only because other things are not, and if we study the conserved things we certainly select. But if we were to study the things which are not conserved, as we sometimes do, we would be exercising selection also. Since the mind is *always* selective, no light is thrown upon the metaphysical status of any entities by pointing out that *they* are selected. The relevant question, the only one of value, is why or in what context the selection is made. Moreover, the thesis that the laws of field-physics are only truisms is a bad pun. The argument is that concepts like matter and energy obey these laws only because matter and energy have been so defined as to obey them. On this argument, every equation, for example, would be a truism, because its roots *when they are found* identically satisfy it upon substitution. But the truism appears only upon a condition; and the condition is

just that one which enables us to say that "the sky is blue" without "sky" and "blue" being identical.

The last four chapters of the book are an argument for identifying the unknown intrinsic natures of sub-atomic entities with qualities akin to mind. The argument is similar to Schopenhauer's identification of the thing-in-itself with the will. Finally, there is a defense of the qualities intuited by the mystic as being genuine qualities of nature. If the foregoing analysis is at all correct Eddington has no right to his conclusions.

2.

Without covering all the ground traversed by his earlier philosophical writings, the present volume[2] contains the essentials of Eddington's familiar interpretation of science—but with not even a shred of comfort for his many critics in the form of retractions from his previously expressed views. Indeed, Eddington is more confident than ever that philosophical idealism is the logical outcome of contemporary physics, and that the "matter" of which physics yields structural knowledge is something akin to consciousness. He is now convinced that quantum laws no less than the laws of molar physics are "mind-made," and that the "objective laws of governance" (if any such laws there be and which at one time he believed were supplied by quantum theory) must be sought in types of behavior not accounted for in the present scheme of physical science—in biology, psychology, life, consciousness, and spirit. The distinctive feature of the present book is its frank epistemological approach, with its insistence that the theory of knowledge should be the point of departure for understanding the nature of the physical world. It maintains that the "fundamental laws" of nature are subjective, that they can be deduced from purely epistemological considerations with the consequence that these laws are both truistic and necessary, and that our sensory and intellectual equipment imposes these laws upon the materials directly accessible for study. This, in brief, is Eddington's philosophy of "selective subjectivism," which without neglecting Berkeley acknowledges its allegiance more definitely to Kant.

2. *The Philosophy of Physical Science.* Sir Arthur Eddington. (Tarner Lectures, 1938.) New York: The Macmillan Company. 1939. ix + 230 pp. $2.50.

However, although Eddington has not been convinced by his critics, his critics are not likely to find the present version of his philosophy more plausible than the previous ones. He insists that the physical universe must be identified as the world "described" by the science of physics (p. 1), and also that epistemological conclusions must be subjected to the same kind of observational control as are the hypotheses of physics (p. 5). But if physics and epistemology are admitted to have the same logical status, why should the deliverances of the latter be taken to be more penetrating and fundamental than those of the former, and why should the "reality" of the physical world be more disputable than is the "reality" of the operations of thought (pp. 3, 18 ff., 23)? Again, what is the sense of calling the "fundamental laws" of nature subjective if, as it turns out, selective subjectivism is a pervasive feature of all knowledge? (Eddington does not illustrate these laws clearly, but declares that they are the "differential equations determining the progress of the universe," p. 63.) For even the features of experience said to be objective (such as the manifestations of consciousness) must also be the outcome of the selective subjectivism of our sensory and intellectual equipment (cf. pp. 114, 148, 195, 217). If the "objective laws of governance" would also have to be presented to us *via* our subjective forms of thought (p. 66), the distinction between subjective and objective has no longer an ascertainable sense, and the terms do not continue to carry the meanings initially associated with them. Eddington maintains that it is not necessarily a disproof of the *a priori* and subjective character of physical laws that they are clearly illustrated by prominent features of the familiar world (p. 133). But if that is so, what *could* disprove this claim, and why isn't Eddington's philosophy of selective subjectivism just a necessary truism, because of the definitions of his terms, with the consequence that it too is only "subjective"? In any case, he can not avoid the paradox of having to impugn the "reality" and the objectivity of our physical knowledge, just because we must *think* in order to know, only to accept the objective reality of philosophical idealism. Physical knowledge presupposes reflective thought and therefore selection. Does Eddington believe that the knowledge claimed by idealism does not? But perhaps that is exactly the not altogether flattering import of his reminder that "we

just can not help being brainy, and must try to make the best of it" (p. 195).

That Eddington's philosophy of science is a consequence of a systematic misuse of terms has been urged against him before (notably by Miss Stebbing, whom he dismisses with a quip), and can be easily established. Thus, to begin with small points, the physical world which was initially identified as that which is "described" by our physical knowledge, is presently said to be an association of pointer-readings (pp. 100-101) presented as the contents of an individual consciousness (p. 195). But the physical universe identified in the first way is a universe containing gravitating bodies, accelerated particles, electrical currents, and the like, and it is only by a sleight of hand that these can be said to be pointer-readings in consciousness. Again, what student of relativity will recognize the essence of that theory to consist in the fact that "we observe only *relations* between physical entities" (p. 31)? Is it very surprising if now the theory of relativity can be "deduced" from the forms of thought? And even though Eddington follows respectable authorities in saying it, can anyone who has ever employed the Newtonian gravitational formula in solving a mechanical problem honestly characterize it as being simply a summary statement of what we have observed (p. 44)?

Eddington maintains that we can be certain of the universal validity of the "fundamental laws" of nature because we ourselves impose them upon experience. But even if the formal deduction of these laws from the forms of thought is granted (in the present book Eddington supplies only the barest outline of the alleged deduction), it is soon evident that this claim means much less than what it at first sight appears to mean. For he readily admits that these laws tell us nothing about the "special facts" of nature—such facts as that the density of matter in many regions of the universe is vanishingly small, or that the earth is spherical. In fact, the interpretation of the "fundamental laws" as laws for familiar physical objects can not be made on *a priori* grounds, as Eddington acknowledges in so many words (p. 134), and the boundary conditions for the differential equations must admittedly be supplied by experimental physics (p. 63). It thus turns out that the "fundamental laws" of nature are simply uninterpreted formulae, and that the *a priori* deduction that Eddington claims

for them is nothing other than the deduction of theorems in a branch of pure mathematics. But it is one thing to make the claim that pure mathematics consists in the deduction of consequences from freely chosen postulates, and quite another thing to maintain that what has been deduced is a law in the positive science of physics.

Eddington's claim to be able to deduce the numerical values of certain "constants of nature" must be adjudged in a similar way. It is certainly possible to determine *a priori* the value of constants: for example, to calculate theoretically the value of the angle-sum of a triangle in Euclidean demonstrative geometry. If then we employ Euclidean geometry as the theory of physical measurement, we may be quite certain that every physical triangle will have its angle-sum equal to two right angles. But issues of empirical fact are not thereby eliminated—they are simply shifted to another place. Whether a given physical configuration is indeed a Euclidean triangle remains a question which involves not merely issues of pure mathematics but also of experimental physics. In general, therefore, in a systematic exposition of a science it is possible to begin with general principles and to show that all the known facts fit into the framework of the theory. But a system of abstract formulae is not a theory of physics, until its terms are interpreted as referring at some point to observational material; and it is not an adequate theory, unless it provides solutions for problems which can be identified without using that theory as the sole mode of identification. For if a theory always determines whether something claimed to be an observational test of the theory is "really" an observation, it is not a physical theory at all, and there is no point whatsoever in conducting experiments for testing it. Not the least puzzling feature of Eddington's views is that they are not obviously compatible with the facts that physics has a history and that experiments continue to be made.

It is now a matter of common knowledge that what appear to be laws of physics may sometimes be "disguised definitions" or "conventions"—even though at one time these laws were empirical generalizations. Such "disguised definitions" are best construed as demands or postulates, in terms of which subject-matter is analyzed and the system of physical knowledge organized. To the extent that Eddington emphasizes this feature of theoretical

physics, and to the extent that he recognizes that such postulates could in principle be retained no matter what the "special facts" may be, his philosophy of science has a sound core. But to build a philosophy simply on this is to build on a half-truth. For in the first place, the conventional status of a principle is a function of the particular organization of physical science at a given time. To say that a law is a convention requires supplementation by a specification of its function in the system of physics—where *the system of physics as a whole* is not accepted on definitional grounds alone. But in the second place, the introduction of a postulate or "disguised definition" is usually made on the tacit assumption that there are in fact measurable properties of bodies which satisfy the conditions stipulated by the postulate. Thus, the principle of the invariance of mechanical laws with respect to certain transformations may be construed as a convention. But its fertility depends on the fact that bodies do have measurable traits and relations which satisfy the indicated conditions, and that these properties (e.g., mass, velocity, etc.), are those in terms of which the behavior of things under discussion can be understood and predicted. It is easy enough to institute conventions, it is not so easy to set up such conventions which will be fruitful in the solution of specific problems. And it is pertinent to note that Eddington himself admits that while the "fundamental laws" are "compulsory," there is no compulsion that actual observations shall satisfy them (p. 20).

It is admittedly important to determine which propositions of the sciences are asserted on factual or contingent grounds, and which are definitory and postulational. But the analysis has not been carried far enough if it terminates in the conclusion that such postulates are adopted simply because of the inherent structure of thought or if it neglects the factual basis of theoretical science. There is just one way to understand without illusion what is the import of the methodological principles of a science—and that is by exhibiting their identifiable functions in inquiry. The source of Eddington's defects as a philosopher of science can be plausibly located in his refusal to perform such an analysis.

14

Probability and the Theory of Knowledge

PROFESSOR REICHENBACH'S WRITINGS have repeatedly called attention to the imporant rôle which probability statements play in all inquiry, and he has made amply clear that no philosophy of science can be regarded as adequate which does not square its accounts with the problems of probable inference. Recently he has brought together in convenient form[1] many reflections on the methodology of science familiar to readers of his earlier works, and at the same time he has set himself the task of solving many well-known problems of epistemology in terms of his theory of probability. His latest book is therefore of great interest, both because of the light it throws on Professor Reichenbach's own views and because it reveals the power and limitations of one approach to the prob-

1. *Experience and Prediction,* Chicago, 1938. All page references, unless otherwise indicated, are to this book.

lems of science. In particular, while it does not add to the details of his theory of probability worked out elsewhere, the applications Professor Reichenbach now makes of it supply fresh clues for judging its import and adequacy. The object of the present essay, therefore, is to expound a number of his views on probability and epistemology, with a view to examining his conclusions and their relevance to the problems he aims to resolve. The discussion will try to determine whether several features of his present views do not follow from assumptions which he has not sufficiently considered; whether his logical constructions do not create new puzzles; and whether a different starting-point should not be taken if the clarification of scientific concepts and procedures, to which Professor Reichenbach's devotion is as unexcelled as it is well known, is to be successfully conducted.

1.

At the risk of repeating familiar things, it will be best to begin with a brief exposition of Professor Reichenbach's general views on probability. They are distinguished by three features: his claim that a *univocal* interpretation can be given to all probability statements in terms of limits of relative frequencies; his introduction of the infinitely gradated property *weight* to characterize propositions; and his consequent proposal to replace the allegedly inadequate two-valued logic of truth and falsity by the infinite-valued logic of probability or weights. These features must now be explained in outline.

Professor Reichenbach takes as the basic type of probability statements those with the form illustrated by: "For every correlated pair x_iy_i (x_i being a man undergoing medical examination at a given time and y_i being that man a year later), x_i is tubercular *implies with degree of probability* p that y_i is dead." He maintains that the triadic relation *probability implication* is a generalization of Russell's *formal implication,* illustrated by "For every x, x is diagnosed as tubercular *implies* that x dies within a year." However, Professor Reichenbach interprets the expression "implies with degree of probability p" as follows: Let h_n be the relative frequency in the first n terms of the series with which the y's die when the correlated x's are tubercular, and suppose h_n converges to a limit p as n increases without limit; p is then the degree of

the probabliity implication. Professor Reichenbach believes that such an interpretation can always be given to probability statements, and maintains that it is the only suitable and adequate one. He also declares that probability statements can be formulated so as to express indifferently relations between classes of "events" (or happenings) or between classes of propositions.

It immediately follows that the attribution of a degree of probability literally to a *single* happening or a *single* proposition is devoid of sense. However, statements which seem to assign probabilities to single happenings do occur and seem to make good sense—as when it is said that it is more probable that Napoleon was ill at the battle of Leipzig than that Caspar Hauser was the son of a prince. Some students have therefore concluded that in such cases a different interpretation must be assigned to the term "probable" than is required on the frequency view, and they have consequently held that the term is *not* univocal. But Professor Reichenbach insists that such a "disparity" view of probability is not necessary. Thus, in the example cited, he regards the difference in probabilities assigned to the two propositions as arising from the difference in the kind of documents which support the two statements; one kind is more reliable because its statements are confirmed with a greater relative frequency than those in the second kind.

For reasons which will be considered presently, no factual statements, according to Professor Reichenbach, can be *known* to be true (or false). Thus, we do not *know* that the next toss with a given die will show a prime number of dots uppermost. Nevertheless, we would be willing to wager on that happening because the probability associated with the *class* of such happenings is high (2/3 in this case). This probability, though strictly applicable to classes of happenings (or propositions), may be taken as the measure of the goodness of a wager on a single happening; when so used, the probability functions as the *weight* of the proposition about this single occurrence. Hence the possible numerical values of the weight are the real numbers in the interval *0* to *1* inclusively; and according to Professor Reichenbach, since the truth or falsity of factual propositions can not be determined in practice, the weights of propositions are the practical surrogates of their truth-values. This is a point of fundamental importance in

Professor Reichenbach's analysis. He maintains that if we assign weights to propositions (in his sense of "weight"), our acceptance of some propositions and rejection of others "can be justified" because in acting upon propositions with high weights "we shall have in the long run the greatest number of successes." (p. 310)

Because the determination of the truth or falsity of propositions is not feasible and because we are compelled to operate with them in terms of their weights, Professor Reichenbach concludes that a two-valued logic is not a satisfactory instrument for science; he therefore proposes an infinite-valued logic of probability, of which the ordinary two-valued logic is claimed to be a specialization. For the details of this new logic the reader must be referred to Professor Reichenbach's writings:[2] its elements are no longer single propositions but *propositional-sequences*, each containing an infinite set of propositions and each being associated with a real number in the interval *0* to *1*. Professor Reichenbach maintains that if this number is interpreted as the limit of the relative frequency with which members of the sequence are true, we obtain the usual mathematical calculus of probability; while if it is interpreted as the weight of any of the members of the sequence we obtain what defenders of the "disparity view" call the logical concept of probability. His conclusion is, therefore, that the mathematical and logical concepts of probability are structurally identical, so that the disparity view is demonstrably mistaken (p. 325). Finally, he develops the idea of probabilities of higher orders, in terms of which he is able to discuss the probability (and so the weight) of probability statements themselves. According to Professor Reichenbach, general no less than singular statements can be characterized in terms of weights, and he therefore argues that scientific theories themselves may be discussed in terms of the frequency theory of probability.

2.

For lack of space this paper will not be concerned with most of the details of Professor Reichenbach's probability theory;[3] but it

2. Especially *Wahrscheinlichkeitslehre*, Leiden, 1935. See also "Ueber Induktion und Wahrscheinlichkeit," *Erkenntnis*, Bd. V, and "Les fondements logiques du calcul des probabilités," *Annales de l'Institut Henri Poincaré*, T. VII.

3. For the sake of completeness, however, I add the following summary of

is essential to examine more closely his conception of weight because of the uses to which he puts it in his discussions of epistemology.

It is evident that the weight of a proposition is determined in precisely the same way in which a probability coefficient is obtained. Now probability is defined in terms of the limit of relative frequencies in infinite series; and it will be therefore obvious that the numerical value of a probability can not be calculated by examining a finite number of elements in such series. In other words, no hypothesis as to what is the probability of some desig-

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some obvious difficulties in Professor Reichenbach's formulation of his system·

a) Probability implication is said to be a generalization of Russell's formal implication. This question has been discussed in some detail by Miss Janina Hosiasson ("La theorie des probabilités est-elle une logique generalisée?", *Actes du Congrès International de Philosophie Scientifique*, Paris, 1935, IV) who points out that in at least three distinct senses of "generalization" this is not the case. Will not therefore Professor Reichenbach indicate in precisely what sense of the term does he believe his claim is true? He has promised to reply to this type of criticism, but at the date of writing this reply is unfortunately not available. But however that may be, his formal calculus is obviously not satisfactorily formulated, since he does not specify all the required rules in terms of which the formalism of probability implication is to be treated.

b) Professor Reichenbach has recently admitted (*Philosophy of Science*, Vol. 5, p. 22) that the many-valued logic as developed in his *Wahrscheinlichkeitslehre* is formulated in the *semantical-language.* He claims nonetheless that the structure of this logic is isomorphic with the structure of the calculus of probability as formulated in the *object-language* of science. But he nowhere gives a proof of this isomorphism, and Dr. C. G. Hempel has recently pointed out ("On the logical form of probability statements," *Erkenntnis*, Bd. VII, p. 157) that this isomorphism does not in fact obtain. The transition from one formulation of the calculus to the other, is not, as Professor Reichenbach apparently believes, simply a matter of "adding quotation marks."

c) Professor Reichenbach insists that his logic of probability is an "extensional system." The point at issue may be only a verbal one. There are, however, standard definitions of the phrase "extensional logic" in the literature, and according to such definitions his system is *not* extensional. It is not clear what Professor Reichenbach means by saying that "a relation is intensional if it depends on the *intension* of the propositions" (*Philosophy of Science*, Vol. 5, p. 25); in any case "intension" is not a term which can safely be used in discussing logical foundations without a more careful formulation of its usage than he gives.

d) Professor Reichenbach claims that his probability logic is a "genuine many-valued logic," if propositional-sequences rather than single propositions are taken as its elements. However, the logical connectives between propositional-sequences are introduced in terms of the familiar logical connectives of the two-valued logic (cf. p. 324). It is therefore not clear why he denies that his many-valued system is built upon a basic two-valued logic in which *propositions* have just two possible truth-values.

nated property can be either conclusively established or conclusively refuted by an inquiry which is necessarily limited to the examination of a finite quantity of evidence. It therefore follows that the weight of a proposition can not be definitely established by any finite number of observations. Two questions thus force themselves upon us: 1) By what procedure does Professor Reichenbach propose to assign weights to propositions? 2) In what way is the weight of a proposition a practical surrogate of its truth-value?

1) Professor Reichenbach's answer to the first question requires a discussion of his Principle of Induction and his theory of higher-order probabilities. Suppose then that in order to estimate the probability of getting a head with a coin we toss it n times, and find that the head turns up m times; the relative frequency of heads in this series of tosses is thus $h_n = \frac{m}{n}$. We do not *know* that h_n is the probability of getting a head, but we may "infer" that it is the limit (or close to the limit). In Professor Reichenbach's terminology, in inferring that h_n *is* the probability we make a *posit* or *wager*, without knowing whether the posit is true. We naturally ask, therefore, what validates this inference. According to him the "justification" is given by the Principle of Induction, formulated as follows: "For any further prolongation of the series as far as s events ($s > n$), the relative frequency will remain within a small interval around h_n, *i.e.*, we assume the relation $h_n - \epsilon \leq h_s \leq h_n + \epsilon$," with the further proviso that there is such an h_n for every ϵ however small (p. 340).

It is evident that this Principle is not demonstrable as a principle of logic or pure mathematics, and Professor Reichenbach stands firmly on the ground that it is not apriori either in a rationalistic or a Kantian sense. How then is the Principle itself to be "justified"? His answer is offered as a solution of Hume's problem: We do not know that the Principle is true; but then we do not know that it is false either. Let us therefore make our posits in accordance with the Inductive Rule: if h_n is the relative frequency with which a property occurs in the first n terms of a series, we shall posit that h_s remains within a narrow interval around h_n for every

prolongation of the series to s terms.[4] Consequently, if the series has a limit, and if its value is estimated in accordance with this Rule—care being taken to correct the value of h as we take more and more terms of the series into account—we have instituted a necessary (though not a sufficient) condition for obtaining the value of the limit; for by taking the latest value of h_n as the best value, we are necessarily approximating more and more closely to the limit if there is one.

Some comments are now in order. a) The above formulation of the Principle of Induction may be misleading if Professor Reichenbach's intent is not made explicit. For example, it is not true that whenever we toss a coin 100 times in 50 of which heads come uppermost, the relative frequency of heads will remain in the interval .50 ± .0001 for every further prolongation of the series of tosses. If the Principle were taken to imply such a consequence it would clearly be false. What Professor Reichenbach means is that for every ϵ *there is an* n such that for every s greater than n, h_s lies in the interval $h_n \pm \epsilon$; but just what the specific value of n for which this is true is left undetermined. It is clear therefore than the Principle asserts nothing more than that the series in question *does* have a limit. b) It follows that even if we make posits in accordance with the Inductive Rule we can not assert, after no matter how many trials, that we *know* what the probability is or, correspondingly, what the weight of a proposition is. No one is more cognizant of this fact than Professor Reichenbach. He points out that in positing a specific h_n as the approximate value of the limit we are making a "blind wager," because we do not in general know *the weight of the proposition which asserts this value.* In proceding according to the Inductive Rule we are therefore employing a self-corrective method, and Professor Reichenbach has performed a valuable service in pointing this out clearly. But does the introduction of that procedure answer the problem which led to its introduction? Are we now in any better position to say what the weight of any specific proposition is?

Professor Reichenbach's further discussion involves the use of

4. *Wahrscheinlichkeitslehre*, p. 397.

higher-order probabilities and what he calls the method of "concatenated induction," with the help of which *blind* wagers are claimed to be converted into *appraised* wagers. This method is said to enable us to "supersede the inductive principle in all those cases in which it would lead us to a false result, or in which it would lead us too late to the right result" (p. 364). We must therefore study it in the context of some illustrations of its use which he offers.

Carbon has not yet been liquefied, although most substances can be melted if raised to sufficiently high temperatures. Is it possible, however, to conclude anything about the fusibility of carbon? Letting "A" stand for the fact that a substance is in a liquid state at a certain temperature and "Ā" that it is not, Professor Reichenbach represents this state of affairs by the following "probability lattice," in which the letters are ordered from left to right according to increasing temperatures:

Copper	Ā	Ā	A	A	A
Iron	Ā	Ā	Ā	A	A
Carbon	Ā	Ā	Ā	Ā	Ā

If an inductive inference is performed from left to right in all but the last line, the conclusion obtained is that the substances in question are in a liquid state when above a sufficiently high temperature; while such an inference performed in the last line yields the conclusion that carbon is not fusible. Each of these inductions is said to be on the "first level." On the other hand, according to Professor Reichenbach, if now an inductive inference is performed in the vertical direction, it leads to the conclusion that carbon will melt at some temperature, because all the other substances do so. This new induction is said to be on the "second level," and Professor Reichenbach declares that "this induction of the second level supersedes an induction of the first level." Each first-level induction is a blind wager, which posits that the limit of the relative frequency of melted states with increase of temperature equals *1* in all but the last row, and posits *o* for the final row. The second-level induction, on the other hand, involves counting the relative frequency with which posits of *1* occur, and itself posits that the probability of posits of o is vanishingly small. In brief, the second-level induction appraises the

weight of each of the blind first-level posits; and according to it the proposition that carbon is not fusible has a weight close to zero.

Let us examine Professor Reichenbach's argument. a) The object of the outlined procedure is to enable us to estimate the weights of propositions. But as he himself emphasizes, the second-level induction involves a blind wager; and even if we "transform" it into an appraised wager by conducting inductions on higher and higher levels, new blind wagers are inevitably introduced. In what way, therefore, are we any better off *after* carrying through any number of steps of such a theoretically endless process than *before*, as far as determining what the weight of a proposition is? Must we not always be content with an *estimate* of a weight without knowing how *good* an estimate it is—unless procedures and assumptions not made explicit in the above account are introduced?[5]

b) In terms of Professor Reichenbach's procedure, the weight of the proposition "Copper has a melting-point" is high, because of the high relative frequency with which statements of the form "x has a melting point" are found to hold. It follows that if this relative frequency were small the weight of that proposition would also be small, even if all the evidence relevant to the behavior of *copper* would confirm its having a point of fusion. To take a similar case, Professor Reichenbach would have to say that "Water

5. Another example given by Professor Reichenbach helps to bring this out even more clearly. He imagines three urns, containing white and black balls in the ratios ¼, ½, and ¾ respectively, but with no indication as to which urn contains which proportion. An urn is now selected, a ball is drawn four times with replacement after each drawing, and it is discovered that a white ball has been picked three times. To the question: What is the probability that in drawing balls from this urn a white one appears? the answer given is ¾, obtained by using the Inductive Rule. This, however, is a blind wager. Professor Reichenbach converts it into an appraised wager, and declares that the probability that this probability is ¾ is equal to 27/46. This last number is a second-level weight, and is said to be greater than the second-level weights for the bind-wagers on ½ or ¼. It is evident that this number is obtained with the help of Bayes Theorem, though Professor Reichenbach does not exhibit his method of derivation, on the assumption that the "initial probabilities" for each urn being selected are equal. How does Professor Reichenbach "justify" this assumption? The reader is told no more than that "further posits and posits of the blind type" must be made (p. 369); and in the more detailed exposition of such "justifications" in the *Wahrscheinlichkeitslehre* (§77), blind posits and new assumptions about the irregularity characterizing hypothetically infinite series are explicitly introduced. The farther one goes along with Professor Reichenbach the more puzzling it becomes why he regards that the problem of determining a weight is solved by him.

expands on freezing" has a low weight, because most substances contract on solidifying. But is "weight" defined as Professor Reichenbach defines it and which leads to such consequences, a term for which physicists, or for that matter students of the methodology of physics, can find a useful application? The contrary seems to be the case. Indeed, the grounds we have for believing in the fusibility of carbon are different from those suggested by the above account; they depend upon the theory of matter we accept because of the *weight of evidence* we have for it, and from which the fusibility of carbon is a consequence—it being understood, however, that "the weight of evidence" for the theory is not to be construed in the way Professor Reichenbach understands his term "weight."

c) Professor Reichenbach's predicate "weight" is the back door through which he reintroduces, in so many words, the conception of the probability of a single case, against which—and rightly so—frequentists have always been up in arms. The discussion thus far has tried to show that in order to evaluate the weight of one proposition, Professor Reichenbach is required to evaluate the weights of an infinite series of higher-level statements. But on his premisses, one must also be prepared to evaluate an infinite series of what might be called *lower-level* weights, before a given weight can be determined. For consider an actual case of estimating a probability, for example, the probability of a normal coin falling heads. We must first count the frequency with which heads fall uppermost in a finite series of throws: we therefore toss a coin on a given occasion, watch it come to rest, note that the head is uppermost, and then continue this process. It would seem, therefore, that we must assert statements like "This coin on this occasion falls heads up." But according to Professor Reichenbach we can not be completely certain of the truth of this statement, since it too possesses a "predictional element"—it possesses for us only a high weight, sufficiently close to *1*, so that we may treat it as true only by schematizing the infinite-valued logic of probability (p. 327). We are therefore led to ask how this allegedly high weight is to be determined. The weight Professor Reichenbach has in mind can not be obtained by regarding the proposition in question as a member of the propositional-sequence each element of which has the form "This coin on occasion x falls heads up"—

for then its weight would be ½ and the procedure would be circular. Its high weight must therefore accrue to it in virtue of its membership in a different propositional-sequence. But which one? Professor Reichenbach is not very confiding on this general point. *Perhaps* the one he has in mind is the class of propositions each of which is a statement by an observer that after the tossed coin has come to rest the head is noted uppermost; the high weight, close to *1*, of the original statement, would then be the value of a relative frequency in this class. If now we ask how *this* ratio is determined, we must count the frequency with which the elements of this class are "true"—a procedure which once more involves a "schematization," since what we call true is again only a high weight. We are therefore led once more to another propositional-sequence, and then to another, and so *ad infinitum.* Even if the suggested propositional-sequence is not the one Professor Reichenbach has in mind, the conclusions drawn from this example would remain unchanged. Does Professor Reichenbach maintain that his use of the term "weight" formulates what is explicitly or implicitly done in the sciences and every-day inquiry, when we try to "justify" the acceptance of propositions which are to be used for action? Or is he *proposing* that such a procedure be instituted as the only one which would "justify" the acceptance or rejection of propositions? If so, does he think that it is a practicable one?

2) We must therefore turn to the second question—in what way is the weight of a proposition a practical surrogate of its truth-value? The gravamen of Professor Reichenbach's objection to the two-valued logic of truth and falsity is that it is not possible to verify completely and finally a proposition as true or as false. But in order to make clear his position, he distinguishes three senses of "possibility." *Technical* possibility involves the matter of fact human ability at a given time to institute and control certain procedures; *physical* possibility involves compatibility with the laws of nature, regardless of human power; *logical* possibility involves no more than sheer logical consistency. Technical possibility does not enter into the discussion of complete verification; on the other hand, while Professor Reichenbach admits that final determination of a truth-value is logically possible, he also believes that the specification of truth-values in terms of such a possibility is not "in

agreement with physics" (p. 42) and should be rejected, because it would permit many sentences to be meaningful which we would normally wish to exclude from being so. What about the physical possibility of complete verification? In the first place, some sentences are obviously not so verifiable, because of the character of the human organism: for example, propositions about a remote future or a remote past, propositions about the internal constitution of the stars, and all universal propositions, including probability statements interpreted in terms of limits of relative frequencies (pp. 40-42); and in the second place, Professor Reichenbach makes clear that even singular statements about the observable present are not completely verifiable either, because they all involve a "predictional component" so that in the context of the system of statements in which they occur they imply an indefinitely large class of consequences about things not directly observable (pp. 50 ff., 85). In brief, therefore, it is not possible to determine with finality the truth-value of propositions. This is the reason offered why we must pay attention to properties of propositions other than their truth-value, if we are going to take propositions as a basis for our actions (p. 66); and Professor Reichenbach maintains that the weight of propositions is just the property which is relevant for our purpose.

He makes it perfectly plain that he regards weight as a property which it is physically possible to determine (p. 54); and his entire system sinks or swims with this contention. But if the previous discussion does not completely misunderstand him, it is evident that the weight of a proposition is no more determinable than is its truth-value. Professor Reichenbach declares that statements about single events at a definite spatio-temporal position might be supposed to be two-valued, but that as a matter of fact "it turns out that a determination remains a mere fiction as we never are able to determine the truth-value in question. All that we can do is make an assumption about the truth-value, based on observation."[6] Is not Professor Reichenbach in exactly the same position with respect to the *weight* of such a proposition? Is he not therefore compelled by his own criteria to regard as a "mere fiction" his infinite-valued logic? It may be granted to him, if for no other

6. *Philosophy of Science,* Vol. 5, p. 28.

reason than for the sake of the argument, that the truth-value of a proposition can not be determined with finality. Will he not also grant that the weight can not be so determined?[7] In any case, there seems no way of escaping the conclusion that *weight*, conceived as Professor Reichenbach does, is no practical surrogate for truth and falsity. He believes that his theory of weights and higher order probabilities solves the problem of the application of the probability calculus to finite classes of elements, if "probability" is defined in terms of limits in infinite series. I would not like to be understood as minimizing the great importance and ingenuity of Professor Reichenbach's contributions to the calculus of probability; but I fail to see why a problem should be considered solved if the proposed solution involves difficulties of increasing complexity and obscurity.

3.

Is it not relevant to ask, therefore, whether the impasse to which Professor Reichenbach's scheme brings us is not a consequence of his assumption that all statements of probability are to be interpreted in exactly the same way? He is on solid ground when he maintains that many such statements require to be interpreted in the way he indicates. However, he also has his eye on the important fact that *all* statements in empirical inquiry are theoretically corrigible by further inquiry, so that statements accepted as

7 Professor Reichenbach might reply that although we can not *know* the weight, we can *wager* on it. Surely. But why can not a like reply be made to his charge that we can not know the *truth-value* of a proposition? Professor Reichenbach seems to me not infrequently to confuse the distinction between a proposition having a truth-value and our knowing what this truth-value is. The real trouble in his discussion seems to me to rest on the fact that he has so defined many of his terms that it is not possible to apply them; in particular, "knowledge," "true," and "weight" are so used by him that as a consequence we can never *know* the truth or the weight of any factual statement whatsoever. Are we not doing some violence to the language when we are compelled to say that we never *know* anything? There would be no point in having the term "know" if we consistently used it that way.

Incidentally, Professor Reichenbach explains that while *truth* involves a relation between a proposition and a fact, *weights* are a function of the state of our knowledge and may vary with the latter (p. 27). But the weight of a proposition, as he defines this term, is as much an "objective" property as truth is, and has nothing to do with the state of our knowledge. Has not Professor Reichenbach slipped into a different conception of weight, at least at this point, from the one he officially avows, and is he not criticizing the two-valued logic from the stand-point of this unofficial view?

true at one time may subsequently be rejected. But he expresses this fact by saying that "all statements are probable," and does not seriously entertain the possibility that this expression may be equivocal. Against those who maintain that this is in fact the case, he declares that those who favor a "disparity" conception of probability violate commonly accepted "principles of empiricism," in particular the principle that all empirical statements be "verifiable" (p. 304). This charge, however, is a gross begging of the question; it involves the gratitous assumption that what some adherents of the "disparity view" call *weight of evidence* or *degree of confirmation* must conform to the same formal conditions of calculation as does probability conceived as a relative frequency (cf. p. 305)—so that as a consequence the only genuine alternative to his own views would be the theory of Laplace and his successors.

In other words, Professor Reichenbach does not seem to be sensitive to the distinction between the (limit of the) relative frequency with which sentences of a certain form are true in an infinite class of such sentences, and the relative frequency with which certain *consequences* of a sentence are true in a finite class of such consequences. The former yields the frequency interpretation of probability statements, which he favors; the latter does not. Without taking the requisite space to develop this second distinction, which leads to the still insufficiently analyzed notion of degree of confirmation,[8] it is worth while to examine Professor Reichenbach's discussion of the problem of induction and the theory of meaning, in order to exhibit the bearing of such a distinction on his conclusions.

1) Professor Reichenbach declares that the aim of induction is "to find series of events whose frequency of occurrence converges toward a limit," even if we have no guaranty that this can be attained (p. 350).[9] The process of induction will therefore termi-

8. This point is briefly discussed in *Principles of the Theory of Probability*, *International Encyclopedia of Unified Science*, Vol. I, No. 6.

9. This formulation seems to me very dubious indeed, unless it is intended as a nominal definition of "induction." Just what is the series of events for which we are supposed to be searching when we look for the individual who committed a crime? Again, when Kepler studied Tycho Brahe's tables on the positions of Mars, what series of events was he looking for? In this case, certainly, most physicists would agree that Kepler was looking for a *formula* (or law), such that the motion of the planet could be specified by it. It is, however, not essential for the purposes of the present discussion to enter into this point at greater length.

nate in conclusions which go beyond the known facts, although the regulative principle in the construction of such conclusions must be "the postulate of the best predictive character" (p. 376). The problem of induction, according to Professor Reichenbach, is to "justify" such conclusions. We must, however, distinguish between the "context of discovery" and the "context of justification." The former involves questions of the psychology and sociology of scientific discovery; the latter, which is alone relevant to the logician of science, "does not regard the processes of thinking in their actual occurrence," and its aim is "to construct justifiable sets of operations which can be intercalated between the starting point and the issue of thought processes, replacing the real intermediate links" (p. 5). The task of epistemology, and of a theory of induction in particular, is to "replace actual thinking by such operations as are justifiable, that is, as can be demonstrated as valid" (p. 7). Such a justification is provided by the Principle of Induction, because if we draw conclusions in accordance with it we shall be following "a procedure the continued application of which must lead to success, if success is possible at all" (p. 377).

One example of the use of this Principle has already been discussed; let us consider another. According to Professor Reichenbach, Galileo's law for freely falling bodies was inferred to hold for all cases similar to those in which confirmatory observations had been made; and Kepler's law of areas was also inferred to hold generally from observations on the spatio-temporal positions of Mars. Finally, Newton's gravitational theory, which "includes the observations of both Galileo and Kepler," made it possible to argue for the validity of each law not simply on the basis of the observational material for each law, but on the basis of the joint observational material for either law. "Newton's discovery, therefore, in unifying both theories, involves an increase of certainty for both of them; it links a more comprehensive body of observational material together to form one inductive group" (p. 372).

Professor Reichenbach does not develop this point further, and it is not easy to see how what he says supports his claim of an "increase of certainty" for the two laws in question. He apparently thinks that because we have made two blind posits on 1 being the limit of relative frequency with which each law is confirmed, and because each law is derivable from Newton's theory,

we are in the position to *appraise* these posits. But why is it relevant that the two laws are each derivable from Newton's theory? Why should we not form "one inductive group" with the observations for Galileo's law and the observations for *any other law*, say Coulomb's, for which the same posit is made as to the relative frequency of confirmations? Professor Reichenbach can not say that the laws of Galileo and Coulomb have "nothing in common" —for as he well knows any mathematician can show the contrary. Alternatively, suppose (what is of course not the case historically) that observations on falling bodies and planets had led us to a posit of .80 as the limit of relative frequency with which Galileo's law is confirmed and a posit of .60 for Kepler's law. Why would the fact that each law is derivable from Newton's theory increase the certainty of these inferences? Professor Reichenbach does not explain such matters. He simply assumes that the "justification" of every theory consists in assigning to it a weight, estimated by repeated application of Inductive Rule. This assumption calls for some comments.

a) In the first place, it does not seem at all plausible that theories, e.g., Galileo's or Newton's, which would be confirmed in say only 80% of their applications would continue to be considered as adequate. It is of course naive to suppose that a single apparent negative instance marks a theory as unsatisfactory; every one knows that a certain range of deviation of observed from expected results is invariably taken into account when testing a theory. There is no such thing as an indubitably final refutation of a theory in the history of science on the basis of isolated negative instances. But having said this, would any physicist retain a theory if a single well-constructed negative instance for it were found—to say nothing of there being an infinity of such exceptions, such a possibility being still compatible with the weight of the theory being *1*? A single negative instance of the kind considered would be a challenge to *alter* the theory, to construct and test *alternative* theories, and not to conduct further experiments for the sake of determining what is the limit of the relative frequency of instances confirming the theory in question. On Professor Reichenbach's view every observed value which departs from an expected value, as calculated from a theory, counts as *one* instance, irrespective of whether it falls inside or outside the range of permitted

deviations. How can he explain the fact that a single deviation falling outside such a range counts for more, in the judgment of workers in the field, than any number of deviations falling inside the range—especially since the value of the limit of the ratio of confirming instances can not be affected by a finite number of disconfirming ones?

b) In citing the power of Newton's theory to unify and thereby strengthen the evidence for the laws of Kepler and Galileo, Professor Reichenbach has made an important observation. But he does not seem to have interpreted his own illustration adequately, and he has not succeeded in showing how the example supports his theory of induction. It seems to me essential to stress the fact that each of these laws is *derivable* from the Newtonian theory, given suitable initial conditions, so that as a consequence the (necessarily finite) confirmatory evidence for these laws taken *singly* also confirms the Newtonian theory. On the other hand, we judge the acceptability of the latter in terms of such factors as the number and variety of the confirmatory instances—in brief, in terms of the "degree" to which the theory has been put to the test. It follows that an observation directly confirming Kepler's law, will also confirm Newton's theory, and will therefore confirm Galileo's law, since an instance confirming a theory also confirms what is derivable from that theory. This account seems to me straightforward though unduly simplified, it involves no *tour de force* such as Professor Reichenbach performs, and expresses what seems to be the procedure of physics, but its cogency depends on not mistaking "probability" as a relative frequency for "probability" as degree of confirmation (i.e., as an indication of the thoroughness with which a proposition has been tested.)[10]

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10. These matters are explained at somewhat fuller length in the work cited in Note 8. Professor Reichenbach distinguishes between two ways of assigning a probability coefficient to a theory, one of which involves viewing a theory as a logical product (p. 396) From the point of view of the proposed distinction between probability and degree of confirmation, his complicated and certainly dubious construction is altogether unnecessary.

[In the second edition of his *Wahrscheinlichkeitslehre* which has since appeared in English (*The Theory of Probability*, Berkeley and Los Angeles: University of California Press, 1949), Professor Reichenbach distinguishes, as he did in earlier publications, between a first-level and a second-level probability of a general law. The first-level probability is the limit of the relative frequency with which the singular consequences of the law (in some cases

c) Professor Reichenbach declares that if we wish to "construct an objective meaning of a term . . . we must ask which meaning we are to ascribe to the term, *if the usual usage of the term is to be justifiable.*" He illustrates his point by explaining the meaning of "length of a line" as the ratio of one line to a unit line; such a meaning "if underlaid to the use of the term in the mouth of most people, makes their statements justifiable." Similarly, he claims that if his interpretation is given to all probability state-

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these singular consequences may all be taken to refer to some one object) are verified. He now considers the objection that on his view a law (e.g., that all human beings have hearts) might be assigned a degree of probability close to one, even though there are a large number of disconfirming instances for it—in opposition to what appears standard procedure which would simply reject the law in such a contingency. He grants the fact that in some cases a single exception to a law would be taken as a "noticeable diminution" of its degree of probability. But he explains this by maintaining that a universal statement is simply a "schematization," involving the substitution for what is actually a "probability-implication" of degree close to one of the more simple and manageable "all-statement." Accordingly, "one exception proves that the strict all-statement is false, and we dislike using an all-statement as a schematization if it is known that the all-statement is false. If a statement is used as a schematization, it should at least be compatible with the existing observational evidence to assume that the schematization is verbally true" (p. 436). But I fail to see that this comment meets the point in any manner. it does not explain why even if no "schematization" is employed and a probability-implication of high degree is asserted, a single exception or even many exceptions should be a ground for rejecting it.

[Professor Reichenbach's discussion of the second-level probability of general statements seems to me no more convincing than his account of first-level ones. By the second-level probability of a law he understands the limit of the relative frequency with which the first-level relative frequencies hold for a series of objects. For example, "In order to define the probability of the second level and thus the probability of Newton's law [of gravitation] in general, not restricted to one test object, we must construct a reference class by filling out the other rows [of the theoretically doubly-infinite lattice] with observations pertaining to other physical laws. For instance, for the second row we can use the law of the conservation of energy, for the third, the law of entropy; and so on. The reference class employed corresponds to the way in which a scientific theory is actually judged, since confidence in an individual law of physics is undoubtedly increased by the fact that other laws, too, have proved to be reliable. Conversely, negative experiences with some physical laws are regarded as a reason for restricting the validity of other laws that so far have not been invalidated" (pp. 439-440). However, unless I completely misunderstand this account, according to it every physical law should receive the same degree of second-level probability; and if this conclusion is warranted, it constitutes in my judgment a *reductio ad absurdum* of the proposed interpretation of the probability of general statements.

[Professor Reichenbach does not regard as relevant criticisms which allege that his analyses do not render the intended meanings of statements ascribing

ments "all our attitude in respect to scientific theories can be justified."[11] This is a puzzling rule for determining the meaning of terms. I had imagined that in attempting to discover what the terms "curvature of space" means in physics, Professor Reichenbach was not aiming at "justifying" the physicist's use of it, but at discovering *how* he does use it. This usage fixes the meaning (or meanings), and it is not easy to see what it would be like to "justify" that usage. Unquestionably, many physicists ascribe a meaning to the term which it does not have; but that it does not have it is discovered by examining how it is used. Assuming such a procedure as basic for discovering the meanings of terms, I can not persuade myself that Professor Reichenbach's interpretation of "the probability of a theory" formulates what physicists do with it or even think about it for the most part.[12] Indeed, he himself admits that the probability of theories, in one of the senses he specifies (i.e., probabilities of the second level), is not yet amenable to quantitative determination, because a sufficient body of statistics is not in existence.[13]

Let us, however, consider Professor Reichenbach's one illustration in which a theory is allegedly assigned a numerical weight,

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a degree of probability to a law; for he declares that he does not pretend to make explicit the meanings actually associated with such locutions, but rather to state what meanings *ought* to be associated with them if our words are to be compatible with our actions (e.g., p. 382). But although he feels entitled in consequence to dismiss as illicit all non-frequency uses of the word "probability" which assign a degree of probability to a statement relative to the available evidence for it, he himself employs a not too dissimilar notion under the label of the "ground-for-assertion relation" in his discussion of the Rule of Induction—even if he denies this relation to fall within the province of the theory of probability and declares it to be "inaccessible to quantitative measure" (p. 460). What this relation is called is of no consequence, and whether it can be metricized (as a number of current writers believe) is perhaps doubtful. But no account of non-demonstrative inference can be regarded as adequate which fails to assign a central importance to it, and it surely merits a fuller discussion than Professor Reichenbach is willing to devote to it.]

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11. *Philosophy of Science*, Vol. 5, p. 33.

12. Professor Reichenbach suggests that those who do not accept his interpretation should ask physicists not what they *mean* by the phrase but what they *do* when they use it (*Philosophy of Science*, Vol. 5, p. 33). Has anyone really done so on a sufficiently extensive scale? It would certainly be interesting and highly instructive to examine the statistics of the replies obtained.

13. *Philosophy of Science*, Vol. 5, p. 41; also p. 399 of the book under discussion.

in his sense of the term. He translates the statement "The velocity of light is 299796 km/sec. with an average error of ±4, or of ±0.0015 per cent" as "The probability that the velocity of light lies between 299792 km/sec. and 299800 km/sec. is 2/3," and correctly interprets the latter in terms of relative frequencies. He then says that from this we can infer "a lower limit for the probability (on the first level) of Einstein's hypothesis of the constancy of the velocity of light." His conclusion is that "the probability of Einstein's hypothesis is greater than 99.99 per cent, if a numerical range of 0.0052 per cent is admitted for the possible value of the constant" (p. 398). This is certainly most impressive. But how are these numerical values obtained? I shall overlook "the transition from the special value of the constant to the postulate of constancy in general"[14] which Professor Reichenbach makes, since it raises a question much too technical to discuss here. As for the rest, these numbers are calculated with the help of the mathematical calculus of probability—applied upon sets of numerical measures *obtained under specified conditions of experiment*, when suitable assumptions are made about the distribution of the measures. Professor Reichenbach's figures thus declare that if prescribed types of measurements are carried out, the magnitudes so obtained will lie within a fixed interval around a certain number with a specified relative frequency. But a study of the procedure by which these figures are obtained reveals nothing relevant for determining the probability of Einstein's hypothesis. It may be that Professor Reichenbach intends his "interpretation" as a *proposal* to the physicist to institute certain practices in evaluating theories; it is most clearly not an account of what the physicist does do.

d) One final point. It is not easy to ascertain what precisely Professor Reichenbach does mean by "justification," or what exactly is the force of "valid" in the passage cited toward the beginning of this section. He says that the construction of the justifying operations is not to be arbitrary, that "it is bound to actual thinking by the postulate of correspondence" (p. 6); and he also says that the construction of theories must be in accordance with the postulate of "the best predictive character." Now a demand for "justification" is readily understood, if it means that evidence for

14. *Ibid.* p. 40.

the theory must be found; such a justification can be given, but only in terms of the consequences or confirmatory instances of a theory and the relative completeness with which a theory has been tested. However, this is not what Professor Reichenbach seems to have in mind, at least it is not the only thing. Thus, he declares that

> "it cannot be denied that there is likewise an inductive relation which supports the theory *before* the consequences are tested. The situation of the theory in respect to facts *before the experimental test* is not different, in principle, from that *after the experimental test.* In both cases there are facts which do not *verify* the theory, but which confer a determinate probability on it; this probability may be small before the test, and great after it. But even before the test there must be facts on which the theory is based; and there must be, also before the test, a set of inductive relations leading from the facts to the theory—else the theory could not be seriously maintained. The set of relations before the test may be of a very complicated kind, whereas the relations from the observational material of the test to the theory may be of a rather simple kind; but in both cases they must be of the inductive type if the adoption of the theory is to be justifiable."[15]

This and similar passages are a puzzle, and it is not altogether clear whether Professor Reichenbach is abiding consistently by his distinction between the context of discovery and that of justification. It is fairly clear, however, that according to him the facts upon which a theory is "based," prior to its being tested experimentally, imply the theory with some degree of probability. Indeed, what distinguishes a great man of science from lesser people is that apparently "he sees the inductive relations" while they do not. But just how are these probability implications to be specified in terms of relative frequencies? Is not Professor Reichenbach flying in the face of our best attested experience as to the historical genesis of theories? Is it at all plausible to believe that "the facts" antecedently to the theory being tested can do more than *suggest* the theory but without determining its specific form? And incidentally, if the alleged probability implication between the facts and theory is a relation involving limits of relative frequencies, how can the probability of a theory be radically altered, as Professor Reichenbach admits it may, by the addition of a *finite* number of confirming instances? On the other hand, many of his

15. *Ibid.* p. 37; also p. 382 ff. of the book.

remarks about the probability of theories become statements of accredited methodological principles, if we interpret them to be about degrees of confirmation. Thus, to "justify" a theory will mean that an appropriate degree of confirmation is to be found, by marshalling a certain finite quantity of evidence to support it; to say that a theory has a "probability" before it is experimentally tested will mean that the "known facts" confirm the theory to some degree, because the propositions asserting them are connected with the theory by a chain of derivations; to maintain that the "probability of a theory" may be made greater by an experimental test will mean, for example, that an experiment, carried on in subject-matter qualitatively different from that in which support has already been found for the theory, will increase the degree of confirmation. Should not Professor Reichenbach therefore give some thought to the notion of degree of confirmation? Will he not contribute his great talents in order to clarify it more fully?

2) The consideration of Professor Reichenbach's discussion of meaning must be brief. The theory of meaning is central to empirically oriented philosophies which take their cue from the procedures of the natural sciences; and at least since Peirce, they have been occupied with setting up criteria for distinguishing meaningful statements from those which are not. It is one of the merits of Professor Reichenbach's discussion that he recognizes that the setting up of such criteria is primarily a matter of making *decisions*, involving questions not of truth and falsity in the first instance, but questions of the practical consequences which follow from making them.

Accepting by and large the standpoint of those who believe there is some connection between the meaning and the verifiability of propositions, Professor Reichenbach first considers what he calls "the truth-theory" of meaning. According to this view a proposition is meaningful if and only if it is completely verifiable as true or false. He points out that such a criterion is very inconvenient, because it is not physically possible to determine the truth-values of most statements with finality; hence such a decision would require us to dismiss as meaningless most of the statements of science. He accordingly proposes a different criterion, which yields "the probability conception of meaning" and is formulated in terms of the weight of propositions instead of their

usual truth-values. On this criterion a proposition has meaning if and only if it is physically possible to determine for it a weight; and two propositions have the same meaning if and only if they obtain the same weight by every possible observation (p. 54).

Is this a suitable criterion? The preceding discussion has shown that the weight of a proposition can be determined with no greater success than its truth-value. It follows that Professor Reichenbach's probability conception of meaning involves the same undesirable consequences as does the truth-theory of meaning. Consequently, if we do not wish to classify as meaningless the statements employed in the sciences, the first part of his criterion must be judged as inappropriate.

Waiving this point, however, we may next ask whether the second part of the criterion concerning the sameness of meaning is adequate for the purposes Professor Reichenbach has in mind. Now suppose that we are about to toss a normal die and that we pronounce the following two statements: "This die will fall with the one-spot uppermost on the next toss," and "This die will fall with the five-spot uppermost on the next toss." Everyone would agree that these statements predict different things, and that no criterion of meaning can be regarded as satisfactory which fails to distinguish between them. But according to the way Professor Reichenbach assigns weights to such propositions, each of them has the weight 1/6, because each is a member of a propositional-sequence in which by hypothesis 1/6 is the limit of the relative frequency with which propositions having a specified form are "true." All possible observations will ascribe the same weight to them, and they should therefore have the same meaning. The proposed criterion for sameness of meaning thus seems to be inadequate.

On the other hand, at least a part of Professor Reichenbach's criterion could be saved, if in his formulation "weight" were interpreted as the degree of confirmation for a proposition. For what is required of statements in the sciences is that from them, with the help of the techniques of logic and in conjunction with other propositions stating suitable initial conditions, *other* statements be derivable,—so that at least some of these derived propositions can be controlled by further observations. In brief, what is required is that statements be *confirmable*, either positively or

negatively, by appropriate observations. Accordingly, a suitable criterion of meaning would seem to be that a proposition be confirmable, or alternatively that it be physically possible to assign a degree of confirmation for the proposition in question. However, it is not clear how a criterion for sameness of meaning is to be formulated in terms of "degrees of confirmation," even if we assume that such "degreees" are always comparable. What we should want to say is that two propositions have the same meaning if all propositions derivable from them which can be directly controlled by observation, receive the *same* confirmation—i.e., such derived propositions are either all positively confirmed or all negatively confirmed. But these speculations are obviously hints for further discussion, and are introduced only for the sake of suggesting that the notion of weight of evidence or degree of confirmation seems to be a fruitful though vague idea, well worth considering as a promising alternative to Professor Reichenbach's conception of weight.

4.

I now turn to Professor Reichenbach's analyses of issues with a distinctively traditional epistemological flavor. In so far as his subsequent discussion depends on his theory of probability, the epistemology he offers falls under the judgment already made upon that theory. It is worth while, however, to consider his epistemological reconstruction of the world on its own merits, even if it should raise old issues of probability theory in a new dress.

1) Professor Reichenbach is not one of those thinkers who dismiss the problem of the external world as meaningless, and not only does he take much pains to defend it as genuine, but he offers a definite resolution of it. What is the problem, according to him? The problem is generated, as always, by the search for *direct* propositions having absolute certainty. Propositions about "immediately observable facts" (i.e., such "concreta" as chairs and tables) turn out to be not completely verifiable, because they involve a predictive element and so are *indirect* and not absolutely certain. But, so the argument runs, in order to confirm, at least in part, such indirect propositions we must finally reach propositions which *are* direct. The view Professor Reichenbach first considers takes these direct propositions to be about *impressions*. Impres-

sions (a term which Professor Reichenbach takes to be synonymous with "presentations," "sensations," and "sense-data") are described as "phenomena occurring within my mind but produced by physical things outside my mind" (p. 89), impressions are "events of my personal sphere" as distinct from "the external world," they do not have "an independent existence," and in observing them "I observe only the impressions produced by" external things on my private world. Since by hypothesis impression statements alone are absolutely certain, the problem to be discussed is "How do we know that there is such an external world outside our private world?" (p. 90). Professor Reichenbach subsequently rejects this hypothesis as to the indubitability of impression statements. He grants it meanwhile for the sake of the argument, maintains that the problem is a serious one, and claims to be able to solve it with the help of his theory of probability. I shall first expound his alleged solution, then argue that it falls short of being one, and finally claim that the "problem" itself is not intelligible.

a) Professor Reichenbach's discussion of the problem is carried on largely in terms of the following analogy. Imagine a cubical world with translucent but not transparent sides, and that its inhabitants note changing shadows upon the walls and ceiling—shadows which *we* know are those of birds flying outside the cube but which are not observable to those in the interior. These shadows stand in the same relation to the external birds as do the private impression to the physical things producing them (p. 115), and the problem to be considered is whether the inhabitants could ever discover that there are things outside their cube. Professor Reichenbach next supposes that some interior Copernicus finally establishes correspondences between shadows on the walls and ceiling, and that he explains these relations by the theory that the moving shapes are shadows of birds existing outside the cube. He also supposes that certain positivistic philosophers reject this hypothesis, declaring it to be a psuedo-statement because the birds are not accessible to observation, and maintaining therefore that the meaning of every statement about the birds is the same as the meaning of sets of statements about the observable shadows. Professor Reichenbach finally adjudicates between the disputants. He declares that *if* the probability-conception of meaning is ac-

cepted, Copernicus is in the right: for his theory has a "surplus meaning," and the weight of the bird-hypothesis is higher than the weight of the mere-shadow-hypothesis. He admits "that the observable facts do not furnish a difference as to the absolute truth or absolute falsehood of the two theories in question; but the weight conferred on them by the facts observable within the cube is different" (p. 121); and he also declares that "what is remarkable here is that the two theories obtain, from the facts observed within the cube, different weights, although both theories furnish for future facts within the cube the same weights . . ." (p. 124). Finally, he insists that

> "Our situation with regard to external things is not essentially different from that of the inhabitants of the cubical world with respect to the birds outside: imagine the surface surrounding that world to contract until it surrounds only our own body, until it finally and with some geometrical deformation, becomes identical with the surface of our body,—we arrive then at the actual conditions for the construction of human knowledge, all our information about the world being bound to the traces which causal processes project from the external things to the surface of our body" (p. 154).

Accordingly, the problem of the external world is solved in the same manner and with the same conclusion as is the predicament of the cube-inhabitants: the external world has an existence of its own, independent of our impressions (p. 129 ff.).

b) Professor Reichenbach's solution hangs upon the validity of his claim that the bird-hypothesis has a higher weight than the mere-shadow theory. Unfortunately, he does not explain how this higher weight can be established, and the only clue he offers is a reference to Bayes Theorem in the calculus of probability. He declares that the "backward probabilities" from the observed facts to the two theories are different, even though the "forward probabilities" from each theory to the facts are the same, because of the difference in the *initial* probabilities of the two theories (p. 124). The argument thus depends on the possibility of assigning probabilities to *theories*, in his sense of the phrase; and reasons have already been advanced why this is not feasible. Waiving this point, however, let us ask how these initial probabilities are to be determined. Now Professor Reichenbach is unquestionably a frequentist in his interpretation of "probability," and these initial probabilities must therefore be estimated by counting

frequencies in material *accessible to those within the cube.* But what sort of frequencies observable within the cube would be relevant to determine the weight of a theory about things not accessible? Professor Reichenbach does not explain this, and lacking such an explanation we might rest our case as to his not having solved the problem until he does so. Let us imagine, however, what his explanation *might* be, though we have no assurance that we read his intent correctly. He might suggest, to put it quite crudely, that the inhabitants would count how frequently moving figures on a screen are shadows of objects behind or before a screen, that the relative frequency with which this is the case is high, and that therefore by an obvious argument from analogy the bird-hypothesis would have a high weight. But on reflection it becomes apparent that the extrapolation from observed frequencies *within* the cube to events inaccessibly *outside* the cube, assumes the very point at issue: the existence of an *outside* to the cube could not be in question if the argument is to mean anything. For what else is involved in the supposition that the inhabitants observe shadows on the *walls* of their world? What would it mean to say that they are hemmed in by "a wall," if the wall did not have an outside as well as an inside? Having assumed so much of a world outside their cube, just what is the problem of the external world to the inhabitants? But without such an assumption, how can the cube-Copernicus, or Professor Reichenbach, even *formulate* the problem? Imagine some terrestrial "physicist" who propounds a theory according to which the familiar objects of common-experience such as tables and chairs, are simply "projections" into our three-dimensional space from things existing "outside" in an inaccessible four-dimensional one. What would Professor Reichenbach's reply be to such a hypothetical physicist who repeated every one of his own arguments for the existence of an external world to establish the existence of such an "external" fourth-dimension? Would he not say that such a "theory" can be intelligibly formulated only if "being external to our space" were given a sense which makes that outside accessible?

Professor Reichenbach's argument does not become easier to understand if we apply it to the problem of inferring an external world from the "personal sphere" of impressions. For just how is one to construe the inevitable task of estimating the initial

probability of the hypothesis of an external world by counting frequencies *within* the surface of the human body? I am forced to conclude, until such time as Professor Reichenbach explains such matters, that he has not solved the problem he set himself. Moreover, I wish to indicate briefly that he does not even have a problem to solve. Impressions are explained to be observable events "of my personal sphere"; and Professor Reichenbach even says that knocking a man's head against a wall is not a good way of persuading him of the reality of the wall, because what the man saw was "outside his body" whereas the pain felt was "inside," so that his problem as to whether there is something "outside of himself" is not thereby answered (p. 92). He does not even hesitate to talk of impressions "being produced" by external things on the "surface of our body." I have not been able to locate any account by him of impressions which does not employ such terms. But if this language is taken seriously, the formulation of the problem of the external world involves the assumption of the existence of such a world. For how otherwise can a distinction be defined and maintained between inside and outside, mine and not mine, on the surface and beneath or above the surface, produced and simply appearing, observed and not observed? An impression cannot be identified as "private" except in the context of things which are *not* private; and a datum of experience cannot be judged as directly observed except in reference to an organism which is not such a datum, and whose introduction therefore assumes the external world the existence of which was allegedly put in question. Without the use of such distinction, however, how can the problem be formulated?[16]

16. Recently Professor E. J. Nelson has formulated the problem in somewhat different language. "By an external world I mean any particular or system of particulars, of whatever nature . . . which is not immediately experiencible. . . . The problem before us then is this: Is it possible to construct a valid inductive argument which concludes from phenomenal evidence or data of experience to the existence of an external world." *Philosophy of Science*, Vol. 3, p. 238. He does not say in what sense of "ible" the external world is not to be immediately experiencible, and his question as it stands is therefore not sufficiently explicit. But in any case, what is to be understood by "immediately"? Does it connote a definite interval of time? If not, how does one go about determining whether anything is immediately experiencible? If so, must not the determination of whether something is immediately experiencible or experienced involve *other* times with respect to which the immediate is defined? And how does Professor Nelson determine whether something is *phe-*

2) Professor Reichenbach's discussion of the problem of the external world is a *tour de force*, almost by his own confession, and serves merely to exhibit the power of his theory of probability. For the discussion was generated by the assumption that impression propositions are absolutely certain. But Professor Reichenbach denies that they have this indubitable character, and considers in detail their more dubious epistemic status. We must examine what he says about them, for his account is by no means clear.

a) His denial that impression propositions are absolutely certain is predicated upon his denial that "impressions have the character of observeable facts." He declares that although he has observed such concreta as tables and houses, he has never seen his "impressions of these things", and to elucidate this point he adds that "I hear tones . . . but I do not hear my hearing of them . . . I feel heat and cold . . . but I do not feel my feeling them" (p. 164). His conclusion is that the existence of impressions is *inferred*, so that impression propositions are *indirect*.

Before expounding his analysis of the chains of inferences involved, we must pause to overcome our surprise at these pronouncements. Professor Reichenbach discussed the problem of the external world on the assumption that impressions *are* observed, for without it the nature of that problem grows even more mysterious. Indeed, the shadows on the walls of the cubical world, which was offered as a precise large-scale model of the epistemological predicament, must certainly be *observed* if there is to be any point to that discussion. His denial just cited therefore indicates some obscurity in his use of the word "impression." On the one hand, he equates it to such terms as "sense-data" and "representations" (p. 165), and because of the use he makes of it in discussing the problem of the external world it is natural to suppose that it refers to *what* is seen, heard, or felt[17] So used it is a

nomenal except in terms of the mechanism of the human body and therefore in terms of something which does not function in that determination as a datum of experience? What then is the problem of the external world, if in order to state it that world seems to be implicitly assumed?

17. In discussing the problem of the knowledge of other minds, Professor Reichenbach says: "Other people tell us that they also see the red and feel the heat and taste the sweet; but we never can compare these *sensations* with ours, and so we do not know whether they are the same"; and again, "It is in a certain sense true that *impressions* of different persons cannot be directly

bit strange to say that impressions are *inferred*, because while empirical evidence supports the claim that colors, sounds, pains, etc., are not psychological primitives, they are nonetheless *not inferred* but are *selected and isolated* in analyses undertaken for definite ends. On the other hand, in the present context Professor Reichenbach is using the term "impressions" to refer, not to what is observed, but to the *act or internal process* of observing. Indeed, he says "An optical sensation is not observed by a man who sees things outside his body; it is inferred. The man *sees* a thing before him and *has* a sensation. . . . He does not know anything about its qualities, except that it has a certain correspondence to the immediate thing he observes. It is an unknown" (p. 237; cf. also pp. 167, 172). But if this interpretation is consistently given to the term, just what is the meaning of the "problem" of the external world and Professor Reichenbach's "solution" of it? And just what is the point of his long argument that impressions, in the second sense indicated, are not observed, and that propositions about them are not indubitable—since impressions in this sense are on par with any objects or processes discussed in physics?

b) Let us, however, resume the exposition. If impressions are not "given," what is given? According to Professor Reichenbach what is given are things or states of things (including certain states of our own bodies); and propositions about them are observation statements with the maximum of directness. From these observation statements we then infer the indirect propositions of physics on the one hand (these are about such things as atoms, electric currents, and the like), and the indirect impression statements on the other (p. 169). The indirect propositions of physics have a weight inferior to the observation statements, because the former have a "surplus meaning" when compared with the latter. But the impression statements, although indirect, have a weight superior to the observation statements, because the former have "less meaning" than the latter (p. 170). Nevertheless, the weights of impression statements are also less than one.

compared. Imagine a man who sees green when I see red, and red when I see green—would we ever know this? A mind untrained in philosophy might perhaps object that the man in question would be in permanent conflict with the traffic regulations when driving a motorcar, that he would cross the street at the red light and stop on the green light—but of course this is thoroughly false" (p. 248, italics not in the text). Here the sense of the passage requires that the italicized words refer to *what* is observed.

These conclusions about impression statements are certainly curious, and Professor Reichenbach's argument for them are no less so. He first claims that impressions must be "described" in terms of physical objects, and that the descriptions take the form of disjunctive propositions. However, in order to obviate the need for enumerating the terms of such disjunctions he introduces the concept of *immediate similarity*. Thus, the table I observe at one time may be *similar* to the table I observe five minutes later "as far as it concerns only what I see just now," irrespective of whether there is a physical identity between the "two tables" (p. 170). Consequently, an "impression of a flash of lightning" can be described by the statement "I had an impression produced by the beam of a light house, or by another physical object which stands in the relation of similarity to such a searchlight." More generally, Professor Reichenbach constructs certain statements he calls *basic*. One kind, called *the longer similarity disjunction*, has the form: "There is the thing *a*, or a thing similar to *a*, or there is no observed physical thing, but only an impression as it would have been produced by the thing *a*"; it may be expressed in the self-explanatory notation: a or $S'(a)$ or $I'(a)$. A second kind of basic statement, called *the impression form*, differs from the preceding in that an "impression term" also occurs in the first two members of the disjunction; it may be expressed in the notation: $[a$ and $I'(a)]$ or $[S'(a)$ and $I'(a)]$ or $I'(a)$. Omitting some inessentials, Professor Reichenbach's argument is as follows: The weight of the longer similarity disjunction is greater than the weight of its first member, by a theorem in the calculus of probability. Secondly, the weight of the impression form is smaller than the weight of the longer similarity disjunction, again by the calculus; nevertheless, "we may conceive it as highly probable that there is always within myself an internal process when I see a thing, and so the weight of the impression disjunction is not much less than the weight of the longer similarity disjunction" (p. 174). Next, by a theorem of formal logic the impression form is equipollent with its own last member. Consequently, the weight of "$I'(a)$" is greater than the weight of "a"; that is, the weight of the observation statement "There is the thing *a*" is *smaller* than the weight of the impression statement "I have an impression of the type produced by *a*." However, basic statements require a com-

parison between a present object and a formerly observed one, so that they involve the reliability of memory which is fallible; it therefore follows that impression statements are not absolutely certain (p. 176).

It is difficult to suppose that in this discussion of impression statements Professor Reichenbach is not shuttling back and forth between the two senses of "impressions" indicated above. Can we readily imagine any one asserting "I have the impression of a flash of lightning" if he did not intend to refer to *what* he sees but to indicate the occurrence of a physiological process under his skin? On the other hand, how can we understand the reason for this complicated argument unless Professor Reichenbach intends such a statement to be about something *not* observed but inferred? But however that may be, his argument depends on some theorems in the calculus of probability, in particular the theorem that the probability of two alternatives is in general greater than the probability of either alternative. Now it is evident that such comparisons can be instituted only if it is assumed that the magnitudes to be compared exist. Professor Reichenbach assumes that weights can be assigned to his longer similarity disjunctions and therefore to impression statements. But how is one to go about estimating the weight for such a statement as "There is only an impression as it would have been produced by a flash of lightning" (p. 178), where by "impression" is understood either an inner process or something seen? What is the propositional-sequence in which such a statement is an element, and how are the relative frequencies within it to be determined? Professor Reichenbach offers no help to his puzzled readers, and produces not one shred of empirical evidence for his contention. Does he really find it plausible that the weight of an observation statement about the coincidence of a pointer and a scale-reading is less than the weight of a proposition about some inner process alleged to accompany such observations? These inner processes are presumably physiological (cf. p. 180), and must therefore be investigated in a way continuous with the way we study the behavior of men in China or the structure of binary stars. Is it all reasonable to assign a privileged epistemic position to propositions about such inner processes? In many inquiries it is usual to isolate certain events such as pointer coincidences, and make them the basis for further inferences because of the reliability of our judgments

about them; but on Professor Reichenbach's account, such a standard procedure of the sciences is quite inexplicable. Again, why does it follow from the fact that memory is in general not infallible, that some *particular* basic statement has a weight different from *1*? Professor Reichenbach does not say, and an enormous gap yawns at this point in his argument. His discussion of impression statements, like his handling of the problem of the external world, thus seems to be primarily an exercise in the calculus of probability, and sheds little light on the concrete procedures of inquiry.

5.

One final portion of Professor Reichenbach's epistemology remains to be considered. The preceding discussions, according to him, have established the following points: that no empirical statements are absolutely verifiable, that to each of them a weight can be attached which is in general less than *1*, and that the observation statements upon which we base all our knowledge are about *concreta*, i.e., "the concrete things of daily life" (p. 195). The remaining constructive task which he sets himself is to exhibit the structure of the inferences from such a basis to the various complex objects of physics and physiology. We shall follow him only over part of his argument, but over a sufficient portion to come to the heart of his epistemology.

a) Although Professor Reichenbach asserts that the basic observation statements are about the concrete things of daily experience, he rapidly disconcerts his readers by first offering a "logical construction" for such objects in terms of material allegedly more primitive psychologically. He begins the discussion by distinguishing between different "kinds of existence."[18] Thus, when at dusk a bush is mistaken for a man, *what* we see at the moment we see it has *immediate existence;* and when we subsequently discover that it is no man but a bush, what we then see also has immediate existence. On the other hand, what is seen in the first instance (i.e., the man) has *subjective existence* only, while what is seen in the second instance (i.e., the bush) has *objective existence.*

However, the determination as to whether what is observed has subjective or objective existence is not a matter for immediate

18. Professor Reichenbach explains that he will use the term "existence" as in modern logic in connection with *descriptions,* not things or individuals, but as will be evident he does not abide by this rule consistently.

intuition, because objective existence is "a determinate logical function of subjective existence" (p. 203). How is this distinction established? According to Professor Reichenbach, the world of immediately existing things is the world of concrete objects around us, "entering into our knowledge without any intellectual operations being performed by us." In this immediate world, said to be historically and psychologically "original" or primitive, there is no difference between waking and dreaming, and "everything exists exactly in the form in which it is observed"; indeed, whenever doubts concerning the *physical* world occur "we go back to the immediately existing objects, to the concreta, as the most reliable facts" (p. 204). On the other hand, we draw inferences from (propositions about) immediately existing things to other such things, having the form "If so-and-so, then such-and-such," some of which are successful while others are not. Accordingly, things immediately existing which do not have the consequences they "normally" have are classified as merely subjective existents—where normality is specified statistically in terms of a high ratio of successful predictions made on the basis of a certain class of immediate things (i.e., those which are then characterized as having objective existence) (p. 207). Since the property of being an objective existent is thus specified statistically, we can never be absolutely certain that something immediately existing is objective. Nevertheless, the weight of propositions about the concrete things of daily life is quite high as a matter of fact, most of the concreta of daily life are "for us, real beyond every doubt, because they have stood up to every test ever applied" (p. 210).

The point of these remarks therefore seems to be that propositions about common-sense objects like chairs and tables, to which objective existence is generally attributed, are themselves "logical constructions" out of propositions about immediate existing things. Such "objects" cannot be judged to be objective existents simply from the fact that something exists immediately. In other words, Professor Reichenbach seems to be subscribing to the sound thesis that prediction of "objectivity" is a case of *knowledge* but that the immediate existence or presentation of anything is *not;* and there is much in his book which supports his taking such a stand. Nevertheless, Professor Reichenbach's position is not unambiguous. Note, for example, his reference to immediate existing objects

as the most *reliable* facts, suggesting that immediate existents are things known or to be known. Note again how simply a problem of knowledge is apparently generated when we ask what the *concreta* are upon which the world is to be constructed as a basis. These concreta are evidently *not* the common-sense objects, the chairs, tables, houses, and thermometers, for as common-sense objects they are already *objective*, and Professor Reichenbach's aim is to "logically construct" them out of more primitive stuff. If, on the other hand, the concreta are immediate existents with a lifetime only during the moments we observe them (cf. p. 199), how can *they* have stood up under *repeated* tests since as immediate existents they are by hypothesis incapable of repetition? And have not the common-sense objects in fact become inaccessible to observation and knowledge, now that the immediately existing things (which Professor Reichenbach will presently tell us are only subjectively existing) are taken to be the appropriate objects of direct knowledge? There is ground for at least suspicion that Professor Reichenbach has on his hand the riddles of a presentational theory of knowledge—suspicions which are permitted to ripen fully in his discussion of scientific objects.

b) According to him, *illata* (i.e., scientific objects such as atoms, electrons, radio waves, and the like) are not concreta because for physical reasons they cannot acquire immediate existence, although it is not *logically* impossible that we should see radio waves "as we see those of light" (p. 215). Nevertheless, we can infer their objective existence as "independent entities," though of course only with probability. Professor Reichenbach's discussion of the relations between illata and concreta must be cited at some length. He first explains that the "concreteness" sometimes attributed to things is mistakenly taken to be restricted to "things of a material existence," because what things can enter the domain of immediate existence is "not at all determined by the place of things in the physical arrangement of the world, but by psychological conditions." He then declares that the perspective in which we see the world from the standpoint of our "middle-scale dimensions" is one-sided.

"We see the world in the scale of our sense capacities: we see houses, trees, men, tools, tables, solids, liquids, waves, fields, woods, and the whole covered by the vault of the heavens. This perspective, however,

is not only one-sided; it is false, in a certain sense. Even the concreta, the things which we believe we see as they are, are objectively of shapes other than we see them. We see the polished surface of our table as a smooth plane; but we know that it is a network of atoms with interstices much larger than the mass particles, and the microscope already shows not the atoms but the fact that the apparent smoothness is not better than the 'smoothness' of the peel of a shriveled apple. We see the iron stove before us as a model of rigidity, solidity, immovability; but we know that its particles perform a violent dance, and that it resembles a swarm of dancing gnats more than the picture of solidity we attribute to it. . . . We do not see the things, not even the concreta, as they objectively are but in a distorted form, we see a *substitute world* —not the world as it is, objectively speaking.

"Using the terminology developed above, we should say that even the concreta are only subjective things, of the type to which an objective thing of different form is coordinated. These things are coupled, but they are not strictly speaking identical. . . . Our immediate world is, strictly speaking, subjective throughout; it is a substitute world in which we live." (pp. 219-220.)

What is the ground offered for these most important conclusions? The reason is that the mechanism of sensation is so organized that it cannot produce a sensation without superimposing upon it a certain "description."

"We do not see things as amorphous but always as framed within a certain description. . . . The objects of our sensations always have a '*Gestalt* character.' They appear as if pressed into a certain conceptual frame; it is their being seen within this frame which we call immediate existence. The description in whose frame we see things corresponds to the objective thing only to a certain extent . . . [and] is never more than a substitute for a completely true description and will express only certain more or less essential features of the physical object." (p. 221.)

The argument has now completed its full period and the bearings of Professor Reichenbach's epistemological principles are apparent. Let us first clear away minor details. Professor Reichenbach began by claiming that the world of immediate existents enters "into our world without any intellectual operations being performed by us." He has now apparently gone back on his word, maintaining that even immediately existing things are overlaid by a descriptional framework which we introduce; the alleged "subjectivity" of what is observed is taken to follow from this fact. On the other hand, he also maintains that the descriptions in whose frame we "see" things correspond to the objective things only to

a certain extent. Now how does Professor Reichenbach know this? What avenue of information has he got about the objective world which enables him to characterize our judgments about the observeable world as "false in a certain sense"? For is not the reason for his discrediting judgments of observation that they are conditioned by the mechanism of the body? Is the knowledge he has of the objective things *independent* of such mechanism—is not this knowledge *also* selective and formulated in a "descriptional frame"? Why should not the knowledge he claims to have of scientific objects be also characterized as not being of objective things, for the very reasons he advances for ruling out the objectivity of shapes and sounds and textures?

Professor Reichenbach also declares that the table *seen* as smooth is not *objectively* smooth. But if the polished table-top is not objectively smooth, to what *can* the term "objectively smooth" be legitimately applied? How would he characterize that table? Surely it is in consonance with established usage to say, and therefore in accordance with the meaning of the phrase, that a polished table-top *is* objectively smooth. If Professor Reichenbach decides that such a table does not have this property, is he not simply *altering* the accepted meaning of the expression, and substituting for it a sense which is different from that ordinarily put upon it, and therefore irrelevant to the specific context he is considering?

There is much in his discussion which suggests that according to him *all* qualities whatsoever have only "subjective existence." Thus, in enumerating the various things that can directly appear as immediate existents he mentions "things, or states of things, including states of my body," but maintains that qualities like red, warm, and hard are *not* given because "they do not belong to the external things"; and he also maintains that science has shown that things have such qualities only when they enter into relations with our own bodies (p. 168). Again, he declares that the fact that we see parallel railroad tracks as convergent "is a subjective fact, since the objective physical stimulus does not give any indication as to this psychological fact" (p. 242). But it is not clear, however, how it follows from the fact that the occurrence of certain qualities is *conditional* upon the occurrence of certain physical processes, that those qualities have only subjective existence. Does not Professor Reichenbach find something curious in an argument

which uses admitted facts of human physiology to support the conclusion that properties observable in physiological processes have only subjective existence? Again, is not the convergence of the parallel rails an objective fact about the rails, the behavior of light being what it is? Or is it Professor Reichenbach's view that an account of the rails in terms of mechanical properties is a "truer" account than one in terms of their optical ones? There is some ground for suspicion that he has simply baptized as "subjective" whatever has *conditions* for its existence, occurrence, or appearance. In such terms, however, what thing or what property of things can rightfully claim objective existence?

Professor Reichenbach's gravest error seems to me to consist in his taking perception as a case of knowledge, contrary to the main intent of his own argument. To prevent the possibility of misjudging him, I shall add one further citation to those already given. In explaining that in spite of the subjectivity of concreta we need not therefore renounce the possibility of ever obtaining a "true picture of the world," he declares:

> "The perspective of the beetle in the meadow is better than ours in the sense that it allows a more precise observation of the individual blades; but the green evenness of the meadow which *we* see is an essential feature also, although unattainable for the beetle. When we see the polished table as a smooth surface, this is not simply false—this picture contains some qualities of the physical table which the picture of the swarm of gnats suppresses, namely, the relative smallness of the corpuscles and interstices compared with the two-dimensional extension. It is true that our substitute world is one-sided; but at least it shows us some essential features of the world. Scientific investigation adds many new features. . . . It is our task to organize all the different pictures obtained in this way into one superior whole. Though this whole is not, in itself, a picture in the sense of a direct perspective, it may be called intuitive in a more indirect sense. We wander through the world, from perspective to perspective, carrying our own subjective horizon with us; it is by a kind of intellectual integration of subjective views that we succeed in constructing a total view of the world, the consistent expansion of which entitles us to ever increasing claims of objectivity." (p. 225.)

He also explains that a logical construction of the world on the basis of immediate existents "allows us to obtain basic statements of a high degree of certainty; for it is much more certain that there is an immediate thing *A* than that there is an objective thing *A*" (p. 266). Indeed, apparently forgetting what he said pre-

viously about impression statements, he declares that the highest initial weights are assigned to propositions about immediately observed things, so that "the most certain knowledge is consequently of the immediate present" (p. 281).

I frankly do not understand Professor Reichenbach if he does not mean to say that the beetle in *perceiving* blades of grass is having *knowledge* of the grass, just as we are having knowledge of it in perceiving the green of the meadow, even though both "views" are subjective. I do not pretend to know what can be meant by having certain knowledge of the immediate present; but I think it is clear that Professor Reichenbach claims such knowledge, and that in making such a claim he is equating the presentation or occurrence of a datum with knowledge. It is useless at this stage to ask how one is to go about estimating the weights of such statements as "There is an immediate thing *A*." It is more important that we conclude this critical study with a few indications of the reasons which have led Professor Reichenbach to his present "dualistic" epistemology.

If cases of perception (or immediate existence) are taken as cases of knowledge, the dualism of a subjective world (into which fall all the familiar common-sense objects) and an objective world (into which fall scientific objects) is generated in a familiar manner. For as Professor Reichenbach repeatedly illustrates, things as perceived do not exhibit properties which things taken as scientific objects are defined to have or are shown to possess by further inquiry. Thus, the perceived optical convergence of rails does not reveal either their mechanical parallelism or their optical parallelism from a different perspective; the observed smoothness of the table-top does not show the complicated molecular structure of the wood; and in general, the presentation of a trait in observation does not disclose the complex of objects which competent scientific investigation establishes as the cause or condition of the existence of that trait. But if perception is knowledge, and if that knowledge is to be veridical, it is natural to argue that traits directly observed ought to be the traits uncovered by the inferential procedures of the sciences. Since, however, this is not the case, and since the explanatory power of scientific objects is obviously superior, it is but a short step to the conclusion that perceptual knowledge is "false in a certain sense," that illata have objective

existence, and that matters directly observed have only subjective existence, especially since the occurrence of perceivable qualities is so clearly conditioned by physical and physiological processes in the perceiving organism. Thus, almost by a play upon words, we are condemned to inhabit a "substitute world," with only problematic means of communication with the objective world of science. Will not Professor Reichenbach be persuaded to abandon the premises from which his dualism follows, and adhere consistently to his genuine insight that thinking as an inferential process is carried on in the objective common-sense world of houses and trees and wars, that it terminates in knowledge of this world, and that "immediate existents" are not objects but instrumentalities of such knowledge? Will he not reconsider his thesis that immediate existents are historical and psychological primitives, and come to think of them as *terminal points* in analysis, discriminated for the sake of making more reliable the application of our theories and of the controls we can institute to test their worth? Will he not consistently recognize, what he does so excellently on occasion, that the identification, differentiation, and location of immediate existents can be performed only in terms of things whose objectivity must be taken for granted? He may then find that the "problem" of adjusting his two worlds, like the problem of the external world, requires no solution because there is no problem to be solved.

One final point. It is not possible to know how seriously Professor Reichenbach intends his language to be taken, and one may easily do him an injustice by taking his words more literally than he planned. But however that may be, he does talk of obtaining a "completely true description" of the world in terms of organizing different subjective "pictures" or "perspectives" into one superior whole. Now it is one thing to organize into a unity the different visual perspectives of a pair of rails; it is another thing to organize into a systematic whole the various physical and chemical theories we may have for the constitution of steel; and it is a third thing, and it seems to me a gravely confused thing, to try to organize into a unified "description" the visual perspectives *and* physico-chemical *conditions* for the occurrence of these perspectives. Rightly or wrongly, Professor Reichenbach's readers do get the impression that according to him a "true picture of the world"

would be a *picture* or *image*, but a picture without perspectives, without focus, and without selection. Would not such a "description" simply *be* the whole world all over again, duplicated to serve a philosopher's fancy? And in any case, is he doing a service to anyone in maintaining that it is not *logically* impossible to *see* atoms or radio waves? It is a question of fact into which we need not enter whether terms like "atom" are not so defined and used that it *is* a logical absurdity to suppose that atoms could be perceived. What is relevant to our purpose is to point out that Professor Reichenbach talks of scientific objects as if *knowledge* of them were a kind of *seeing*, and that in so doing he has not clarified what terms like "atom," "radio wave," "light wave" *mean* because he has not exhibited how such terms are *used* in theoretical physics and scientific inquiry. Thus, his defense of the "independent existence" of atoms is effective enough against those who deny such existence; but both the defense and the offense are carried on upon the common assumption that the atomic theory is a pictorial report or description of matters which, alas for our human limitations, are not accessible to direct observation through the senses. Is it not high time to challenge this assumption and to insist that having knowledge is nothing like having an image, however grand and unified such images may be? And is not the way to discover what knowledge of the existence of atoms and light waves is, that of examining the *procedures* of the physicist when he claims such knowledge and of the *uses* he makes of these terms in his inquiries? Having now made his venture into epistemology, will not Professor Reichenbach return to the kind of analyses he was performing when he wrote his *Axiomatik der Raum-Zeit Lehre* and his *Philosophie der Raum-Zeit Lehre*? It is in such works that the strength and virtue of scientific philosophy lie.

15

Sovereign Reason

"It is a habit of mankind to entrust to careless hope what they long for, and to use sovereign reason to thrust aside what they do not fancy."—THUCYDIDES.

I.

THE INJUNCTION to take Reason as one's guide has fallen repeatedly on men's ears at least since classical antiquity; and the place of Reason in nature and society has been an oft-recurring theme in the history of thought. Philosophers have been perennially occupied with analyzing Reason's anatomy and function; poets and moralists have celebrated its worth and dignity; and even those engrossed in the conquest and cynical use of power have not seldom made formal obeisance to its authority. If men have so often been found wanting in adapting their conduct and beliefs to the precepts of Reason, it has surely not been for lack of frequent verbal encouragement to do so.

Men's failure to live reasonably is in large measure a consequence of the fact that though man is by nature a rational animal, the proficient exercise of rational powers is not a natural blessing but a difficult achievement. For the full realization of those powers is the end-product of an arduous personal discipline, to which only a few of mankind have been able to subject themselves; and the exercise of those powers is also contingent upon favorable material and social circumstances, not easily called into existence, and not generally available in human epochs when a rigid tradition or brutal compulsion is the primary determinant of personal and social action. However, mankind's alleged failure to take Reason for its guide cannot always be explained in such terms. For the allegation is often the consequence of the fact that human beliefs and actions are being judged on the basis of conflicting conceptions as to what it is *to be rational.* Though the name of Reason is frequently invoked to sanction or to condemn various practices and beliefs, Reason's ostensible spokesmen do not always speak with a single voice. In short, men have been confronted with incompatible ideals of Reason, not all of which can be congruent with human powers and nature's organization. Whatever else one may learn from the study of philosophy and its history, one cannot easily escape the fact that the canons which mankind has employed in evaluating the reasonableness of conduct and belief have varied with local tradition and historical circumstance.

Systems of philosophy can indeed be profitably studied as explicit and refined formulations of standards of rationality, proposed as ideals against which conduct and claims to genuine knowledge are to be measured. These standards, whatever may be the evidence on which their proponents formally accept them, have in general not been the exclusive creations of those who thus explicitly propose them: they have frequently been the symptoms and intellectual expressions of pervasive tensions and needs operating in various forms in the inclusive social matrix within which philosophical systems come to birth or find wide acceptance. It is thus undoubtedly illuminating to examine philosophical doctrines in terms of their origins and causes and to determine to what extent the perspectives from which philosophers view the world are formed by current customs, current beliefs, and current moral and intellectual problems.

However, philosophical ideas are not simply otiose by-products of cultural processes, exercising no reciprocal influence upon their matrix cultures. On the contrary, the standards of rationality explicitly or implicitly contained in philosophic systems have repeatedly served as guides in resolving practical and intellectual issues and in directing theoretical inquiry. The general acceptance of a system of philosophy thus frequently leads to consequences of far-reaching importance to both society and science. Accordingly, the analysis of the canons of rationality involved in philosophic doctrines, with a view to evaluating their adequacy and authority, as distinct from investigating their causes and their matter-of-fact consequences, is a contribution, however indirect, to serious social criticism.

It is with such an examination of the canons of rationality implicit in one historically influential system of philosophy—that of philosophical idealism—that the present paper is concerned. Few would care to deny that the domination which this system of ideas once exercised over thinking minds in this country is now a thing of the past. Nevertheless, the conception of Reason proposed by it is still worth serious attention. It is a conception which satisfies the deep-seated need of sensitive minds for an ideal that is inclusive and worthy of human devotion. It is a view of the goal of thought and of the power of reason that appears to many as the sole alternative to accepting dogmatic preference and brute power as ultimate standards. And it is an interpretation of the office of Reason which still controls the minds of many men eminent in science and art, and which in various guises guides much recent discussion in social and moral philosophy. One is therefore not engaging in a gratuitous intellectual exercise in attempting a fresh evaluation of this ideal of Reason. Is it an ideal firmly rooted in the character of the world and implicit in the actual operations of human reflection? Or is it a conception of the goal of thought that is fundamentally irrelevant to the procedures and conclusions of controlled inquiry, inherently incapable of even partial attainment, the vain pursuit of which leads only to an enervating scepticism and eventual despair?

This ideal of Reason has been argued in recent years with great vigor and unusual clarity by Professor Brand Blanshard. His presentation of the case for it has in addition the merit of recognizing

many of the deficiencies and obscurities of earlier formulations of the doctrine and of exhibiting considerable familiarity with relevant developments in modern logic and science. The task of evaluating the standard of rationality proposed by philosophic idealism has thus become a relatively easy one, for it is now possible to concentrate on Mr. Blanshard's presentation of the evidence for this ideal, in the confident belief that no presentation of the case for it more cogent than his could be made. Accordingly, the present paper will be devoted exclusively to appraising some of the considerations advanced by Mr. Blanshard in favor of this ideal. It will be impossible, naturally, to deal with all the issues he raises in all their dimensions, and in particular it will be necessary to pass by in silence his own criticisms of alternative standards of rationality that have been proposed by contemporary pluralistic naturalists. But while much in his argument must be neglected, it is hoped that what will be discussed is at any rate near the center of his vision.

What, then, is the proper goal of Reason, as Mr. Blanshard envisages it, and what are the arguments upon which he rests his case? The very end and goal of Reason, he declares,

> is to understand, and to understand is always to follow an objective pattern or order. What kind of order is this? If it is to satisfy reason, it must be an intelligible order, and what is that? It is an order that never meets our question Why? with a final rebuff, one in which there is always an answer to be found, whether in fact we find it or not. And what sort of answer would satisfy that question? Only an answer in terms of necessity, and ultimately of logical necessity, since of any answer that falls short of this the question Why? can be raised again. When we reach an answer that is necessary, we see that to repeat the question is idle. Of any statement of merely causal necessity, such as the law of gravitation, or Ohm's law, or Boyle's law, we can intelligibly ask why things should behave in this manner. But when we see that things equal to the same thing are equal to each other, we cannot sensibly ask why, because we are at the end of the line to which such questioning can take us. We have already reached the logically necessary.[1]

And as he explains more fully elsewhere,

> Fully coherent knowledge would be knowledge in which every judgment entailed, and was entailed by, the rest of the system Probably we never find in fact a system where there is so much of interdepen-

1. "Current Strictures on Reason," *Philosophical Review*, LIV (July, 1945), 360-361.

dence. . . . It is in such systems, perhaps, as Euclidean geometry that we get the most perfect examples of coherence that have been constructed. If any proposition were lacking, it could be supplied from the rest; if any were altered, the repercussions would be felt through the length and breadth of the system. Yet even such a system as this falls short of ideal system. Its postulates are unproved; they are independent of each other, in the sense that none of them could be derived from any other or even from all the others together; its clear necessity is brought by an abstractness so extreme as to have left out nearly everything that belongs to the character of actual things. A completely satisfactory system would have none of these defects. No proposition would be arbitrary, every proposition would be entailed by the others jointly and even singly, no proposition would stand outside the system. . . .[2]

But this ideal of knowledge is a valid ideal, and something other than the expression of undisciplined self-indulgence, Mr. Blanshard believes, only if the world which thought seeks to apprehend is one "in which intelligence finds an answering intelligibility,"[3] only if reality likewise is an "all-inclusive and perfectly integrated system"[4] whose parts logically imply each other. It is upon this view of reality that he ultimately rests his case for the ideal of reason he professes, and it is to a defense of this theory of reality that he devotes his best efforts.

At this point, however, a reader not antecedently committed to Mr. Blanshard's canons of intelligibility might enter a protest. Why should the goal of human reason, such a reader might ask, be dictated by this alleged character of reality, even if this all-inclusive reality does have the character Mr. Blanshard believes it to have? What logical compulsion is there that even if the world does possess such a perfectly integrated logical structure, human thought should seek to encompass it? Are not the actual tasks of human reason set by *specific* problems, involving only a sector of what exists, whose successful resolution does not, in point of fact, require a consideration of the rest of nature? And should not, therefore, the ideals of human reason, and the principles of criticism that men ought to employ in evaluating proposed solutions to their problems, be established by considering the ways in which specific problems do become resolved, rather than by trying to

2. *The Nature of Thought* (New York, 1940), II, 264-266.
3. "Current Strictures on Reason," p. 361.
4. *The Nature of Thought*, II, 475.

ground those ideals and principles in the character of an all-inclusive reality that is only vaguely present to men's vision?

Mr. Blanshard is not entirely unmindful of such objections, nor does he conceal from himself that the ideal of knowledge he portrays is in conflict with the general positivistic tenor of modern science as well as with many current naturalistic interpretations of the function of thought. Nevertheless, he attempts to show that the ideal he invokes for human reason is implicit in the tasks human reason normally undertakes. He rejects the view, common since Hume, that all propositions about matters of fact are contingent; for he maintains and tries to prove that "in the end" no propositions are devoid of logical necessity, and that only on the supposition that no proposition is "altogether contingent"[5] can responsible inquiry be distinguished from arbitrary postulation. Objections of the type briefly cited above do not therefore lead Mr. Blanshard to doubt his fundamental commitments, and a critic who wishes to come to grips with him must consider the positive arguments he employs to support his thesis. The foundation upon which Mr. Blanshard builds his case is the doctrine of internal relations, and it is with an examination of his arguments for this doctrine that the remainder of this paper will be concerned.

2.

The issue raised by the doctrine of internal relations is "whether a term could be what it is apart from the relations it bears to others." Mr. Blanshard explains this issue more fully as follows:

> A relation is internal to a term when in its absence the term would be different; it is external when its addition or withdrawal would make *no* difference to the term. . . . Those who accept the theory of internal relations . . . hold that everything, if we knew enough, would turn out to be internally related to everything else. . . .[6]

But this formulation, he believes, is not free from ambiguity, and he therefore amplifies it into the following statement:

> . . . Everything is so integral a part of a context that it can neither be nor be truly conceived apart from that context. Put more formally, the theory is this: (1) that every term, i.e., every possible object of thought, is what it is in virtue of relations to what is other than itself; (2) that its

5. "Current Strictures on Reason," p. 368.
6. *The Nature of Thought,* II, 451.

nature is affected thus not by some of its relations only, but in differing degrees by all of them, no matter how external they may seem; (3) that in consequence of (2) and of the further obvious fact that everything is related in *some* way to everything else, no knowledge will reveal completely the nature of any term until it has exhausted that term's relations to everything else.[7]

Mr. Blanshard's statement of the doctrine of internal relations has undoubted advantages of greater clarity over other formulations. Nevertheless, one crucial point in it seems to me essentially obscure, and something must be said about it before I turn to his detailed defense of his thesis. For in the statement of his doctrine as well as in the ensuing discussion of it Mr. Blanshard uses the phrase "the nature of a term," though I have been unable to discover in his writings any explicit explanation of what he means by it; yet everything depends upon the sense that is to be assigned to it. What ought we to understand by the expression?

At least three uses of the word "nature" can be distinguished when the word occurs in such contexts as "the nature of *x*."

(1) In the first place, it frequently occurs in questions and answers such as the following: "What is the nature of a circle?"—"The nature of a circle is to be a closed plane curve, all of whose points are equidistant from a fixed point"; "What is the nature of electricity?"—"The nature of electricity is to be a mode of physical behavior specified by Maxwell's equations"; "What is the nature of man?"—"The nature of man is to be a rational animal." It is clear that in this usage, the terms whose natures are being discussed are *kinds, characters,* or *universals,* are capable of repeated exemplification in concrete individuals and processes but are not themselves concrete individuals or processes. Successful inquiries into natures, in this sense of the word, terminate in what has traditionally been called *definition,* though the outcome of such research might more appropriately be designated as *theory.* The intellectual service that is rendered when the natures of universals are satisfactorily formulated is that other generic characters associated with the former can be exhibited as logical implicates of those universals.[8] I shall call this use of the word "nature" its *primary* use.

7. *Ibid.*, p. 452.

8. Although I am employing realistic language throughout this paper, and am thus assuming that there are such things as universals, this is done primarily

An allied sense of the word "nature" is illustrated by such statements as "The nature of gold is to be malleable" and "It is the nature of cats to catch mice." Here again natures are predicated of universals or characters, not of individuals. However, what is said in such statements to be the nature of a kind is not a definition or complete theory of the kind but is regarded as merely a logical implicate of some assumed complete theory. Thus, the dispositional predicate "being malleable" is generally not taken to constitute the definition of gold, though it is commonly supposed to follow logically from such a definition.

(2) A second important sense of the word "nature" is illustrated by such statements as: "It is the nature of this particular figure to have an angle-sum equal to two right-angles," "To be rust-resisting is the nature of this knife," or "The nature of Socrates is to be mortal." In these contexts natures are being predicated of concrete things or individuals, rather than of universals or characters as in the previous examples. However, statements like the present ones must frequently be understood as elliptic formulations, in which something is predicated to *be* the nature, or to be *of* the nature, of an individual, only in the sense that the individual has the character designated as its nature as a consequence of his displaying some *other* character. Thus, the mortality which is asserted to be of the nature of Socrates belongs to him *insofar* as Socrates is human, for if Socrates is a man, and assuming an appropriate formulation of the nature of the generic character *man* (in the primary sense of the word "nature"), it follows that Socrates is mortal. In this usage of the word "nature," therefore, to be snub-nosed is not part of the nature of Socrates, since though he is a man, it does not follow from his being one that he is snub-nosed. In this usage, also, though to be mortal is of the nature of Socrates insofar as he is human, mortality need not be part of his nature insofar as he may exhibit some other generic character, for example having a physical body of a determinate shape. Accordingly, when, in the present sense of the word "nature," an individual is said to have a specified nature, what is being asserted

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for the sake of expediting the present discussion. Whether, and in what sense, one *must* make this assumption is another question, whose resolution does not, I think, affect the argument in this paper.

is a connection between characters or universals. Since, however, an individual possesses an indefinite number of characters, not all of which logically entail one another, whether a given trait the individual exhibits does or does not belong to his nature is relative to what other character is selected for describing the individual.

(3) I come finally to a third and most puzzling use of the word "nature," according to which individuals are said to have intrinsic natures, where the predication of such natures is supposed to be made without ellipsis. Thus, it is frequently said that the nature of a given individual (e g., Socrates) is to be a man, or that the nature of the moon is to be a satellite, not insofar as those individuals exhibit some further character, but absolutely and without qualification. What are we to understand by the word "nature" when it is used in this manner?

There is one interpretation that seems obvious, though it may not carry us far. On this interpretation the character attributed to an individual as its nature is one which permits the systematic organization and logical derivation of a large number of other traits the individual exhibits. For example, in asserting that the nature of Socrates is to be a man, what we are asserting on the proposed interpretation is that many other characters possessed by Socrates, such as the ability to see and hear, to experience joy and sorrow, to resent injury, to remember and reflect, are logical consequences of his being a man. However, this interpretation of the word "nature" does not require us to say that *every* trait an individual thing possesses is a consequence of its nature. Thus, even if on some theory of man it would follow from the fact that Socrates is a man that he must be capable of sexual passion, it would not follow from his *nature alone* that he must be fond of music, or that he must be a lover of Alcibiades. In brief, on this interpretation of the term, in predicating a character of an individual as its nature we bring into systematic order only a *selected* group of traits and actions it exhibits.

It is at this point that our difficulties begin, for there apparently are some people, among whom Mr. Blanshard is perhaps to be included, who conceive the nature of an individual as something which logically determines *all* the thing's attributes and relational properties and not merely *some* of them. But such a use of the word "nature" seems to me to lead to fatal consequences.

In the first place, it is quite clear that just what characters are included in an individual, and just where the boundaries of an individual are drawn, depend on decisions as to the use of language. These decisions, though motivated by considerations of practical utility, are *logically arbitrary.* Thus, the expression "the sun" is generally understood to cover an object confined to a certain apparent volume possessing a certain shape, and exhibiting certain radiant properties; it is usually not employed so as to cover the innumerable spatial relations that object has to other things, nor the energies that had been radiated from the object but are now millions of miles away from it. Nevertheless, the phrase "the sun" *could* be used so that the individual thing to which it refers will include not only the items just mentioned but also all the physical events that stand to the thing (as initially specified) in relations of causal antecedents and consequences, and even all the images and ideas which men have had or ever will have of it. Accordingly, just what qualities and relations are to be included as parts or elements of an individual thing is not a question to be settled by empirical investigation of facts, but a question which calls for a practical delimitation.

However, if the word "individual" is so used that an individual will include *all possible* attributes, relations, and relational properties it may possess, two consequences immediately follow: there will be only one individual, which will coincide with the conjectural "totality" of all things, events, and relations; and secondly, every statement containing the name of an individual will express an analytical proposition. Both consequences are practically undesirable, for reasons too obvious to need mention. But these consequences can be avoided only by restricting the use of the word "individual," as is normally done, so that individuals will include only a proper subset of their possible attributes and relational properties, however vaguely this subset might be delimited and however inexhaustible its membership may be.

But if this is so, and if, as is generally admitted, in the normal use of the word "individual" individuals are not logically definable (because they are taken to include an inexhaustible set of *logically independent* characters), what are we to understand by assertions concerning the nature of an individual thing, in the absolute, unqualified sense of the word "nature"? We must remind ourselves

that, in this absolute sense, the nature of an individual is supposed to determine *logically all* the thing's traits and behaviors, its enduring as well as its passing qualities. On the other hand, it is demonstrable that if the nature of a thing is something that is capable of formulation and definition, the nature of a thing cannot by itself determine *all* of its characteristics. For example, if to be a metal is taken to be the nature of a concrete thing, this nature *may* entail the fact that the thing is malleable; but this nature will not, by itself, determine the specific degree of malleability exhibited by the thing, nor will it determine the specific shapes the thing may assume at various times. Such further statements about the thing are derivable from a statement about its nature only if the latter is supplemented by other, logically independent statements, which are instantial in form and specify the contingent initial and boundary conditions under which the thing happens to exist. Accordingly, on the supposition that the natures of things are statable and definable, the nature of a thing cannot determine *every* character the thing may possess.

As far as I can see, however, this conclusion can be avoided in only one way—by equating the nature of a thing with the thing itself. But such an attempted escape from difficulties leads to consequences no less disastrous. In the first place, the nature of a thing, like the thing itself, would be something that is in principle indefinable and could not therefore be made the basis for bringing into systematic order any of the characters which the thing displays. In the second place, every statement which mentions the nature of an individual would express no more than a trivial analytical proposition. And in the third place, since discursive thought would be inherently inadequate to the task of discovering the natures of things, the goal of understanding the natures of things could not be a pertinent ideal for human reason.

Should these difficulties be brushed aside with the comment that they arise only for finite minds and not for an "infinite intelligence," the appropriate rejoinder is close at hand. Why *should* finite minds adopt an ideal of reason that is suitable for an intelligence totally different from theirs? Moreover, would not even an all-encompassing mind fail to achieve the "fully coherent knowledge" that Mr. Blanshard envisages as the ultimate aim of thought? For the characters things possess fall into a large number of sub-

classes which are demonstrably independent of each other *logically*. If, therefore, an infinite mind did ever come to know the nature of a thing, it would know it only as a miscellaneous collection of attributes and relational properties, some of which do logically entail others, and some of which are logically independent of others. Accordingly, even such a mind would be compelled to recognize an ineradicable contingency in the very heart of the nature of things.

I have spent much time on matters that are preliminary to a discussion of Mr. Blanshard's arguments for the doctrine of internal relations. I hope the time has not been misspent. Mr. Blanshard nowhere states explicitly what he understands by the phrase "the nature of a term"; but his rejection of the false or abstract universal in favor of the true or concrete universal suggests that for him the nature of a thing simply *is* the total set of characters included in the thing. Indeed, he does say that

> the nature of any term, unless the term is itself a relation, consists of attributes or properties (in the non-technical sense), by the nature of an apple we mean its roundness, its redness, its juiciness, and so on. Thus a change in any of the properties would be a change in the apple's nature.[9]

And such a statement does provide some ground for the suspicion that this is the way he is using the expression "the nature of a thing." But in any event, I shall try to show that only on such an interpretation of this expression do his arguments for the doctrine of internal relations fail to illustrate the fallacy of *non sequitur*.

3.

I shall examine Mr. Blanshard's arguments for the doctrine of internal relations under three divisions into which they can be conveniently placed: arguments concerned with the relations of concrete things to one another, those dealing with the relations of universals; and finally, those addressed to the nature of causal relations.

Although Mr. Blanshard offers several grounds for his theory under the first head, they seem to me to be homogeneous in type, and I shall therefore comment on only one, in the belief that it is representative of the others. According to this argument, "everything is related to everything else by the relation of difference at

9. *The Nature of Thought*, II, 478.

least," so that if *A* and *B* are two concrete (and therefore distinct) individuals, *A* must be related to *B* by the relation of *difference.* However, were this relation altered, *A* would no longer be the thing it is, since it would then not differ from that which, by hypothesis, is distinct from itself. But "a relation that could not be theoretically changed without changing the thing itself is precisely what we mean by an internal relation."[10] Hence the relation of difference is internal to *A;* indeed, everything is therefore internally related in this manner to everything else. And by a strictly parallel argument Mr. Blanshard also tries to show that "what holds in this respect of the relation of difference holds of other relations as well."[11]

I fear, however, that though this argument has the impressive quality of great simplicity, its only merit is that it establishes a truism. To show that this is so, I shall restate it in terms of a special example. Suppose *A* and *B* are two individual plane figures, *A* having the shape of a circle, *B* that of a triangle. *A* and *B* are surely different, both numerically and with respect to the shapes they possess. Mr. Blanshard's claim is that the relation of being different from *B* is internal to *A*, because if *A* did not stand in this relation to *B*, *A* would be different from what it in fact is. Does the argument establish what Mr. Blanshard believes it does?

(a) Notice, in the first place, that if the nature of a thing is distinguished from the thing, the admitted facts of the example do not yield the conclusion that the relation of difference is internal to the individual *A*. Undoubtedly, given that *B* is triangular in shape, *A* could not be circular unless *A* differed in shape from *B*. But to say that *A* would fail to have the shape it does in fact have, did it not differ in shape from *B*, is *prima facie* not equivalent to saying that *A*'s *nature* would be affected were *A* not different in shape from *B*. However, on Mr. Blanshard's explicit formulation of the doctrine of internal relations, it is this *latter* claim that must be made good if the relation of *difference* is to be established as internal to *A*. But Mr. Blanshard offers no reasons why his readers should accept this claim, unless indeed he assumes, contrary to the hypothesis, that *A* and *A*'s nature are one and the same. If, however, he does assume this, the relation of difference

10. *Ibid.*, pp. 476-477.
11. *Ibid.*, p. 478.

is internal to *A*, but only because of some initial (though perhaps not explicit) practical decision as to what attributes and relational properties are to be included in the individual *A*. Accordingly, Mr. Blanshard has supplied adequate grounds for the statement that difference is a relation internal to *A* if, and only if, this statement is construed as a glaring tautology.

(b) Let us consider the matter in another light. Suppose that the individual figure *B* were to be destroyed so that *A*, though retaining its circular shape, would no longer be different from *B*—for the simple reason that there no longer would be the figure *B* from which it could differ. It seems, therefore, that *A* remains the thing it is in spite of the fact that one of its relations is altered.

I do not know what Mr. Blanshard would say to this objection to his argument, but his reply might conceivably take the following form: To be sure, he might say, the shape of *A* need not be affected by the destruction of the individual *B*, but its *nature* would be. For the nature of *A* is something such that the fact that *A* stands in some relation *R* to a thing follows logically from that nature. But since, on the hypothesis under discussion, *A* ceases to have a relation to a thing that it did have, *A*'s nature must be acknowledged to have undergone alteration, on pain of logical contradiction.

If this is Mr. Blanshard's reply, he requires us to consider again the cryptic notion of the nature of an individual thing. Now it is certainly the case that the proposition, *A* is different in shape from *B*, follows logically from the two propositions that *A* is circular in shape and *B* is triangular, where *A* and *B* are two plane figures. But it is well to note that the conclusion of this inference is entailed by propositions about the *shapes* of the two individuals. If, then, the relational property of being different from *B* is alleged to be internal to *A*, it is internal to it only relative to the contingent facts that *A* is circular and *B* is triangular. Accordingly, to assert that *A* is necessarily related to *B* by the relation of difference, is simply an *elliptic* formulation of the fact that the *characters A* and *B* possess logically exclude one another. On the other hand, neither the proposition that *A* is circular in shape nor the proposition that *B* is triangular in shape is logically necessary, and we cannot therefore conclude that the relational property of being different from *B* is internal to *A* without further qualification. On

the contrary, though the relation of difference may be internal to *A* relative to *A*'s possessing one character, it will not in general be internal to *A* relative to *A*'s possessing some other character. For example, if *A* and *B* are figures constructed out of white chalk, the relational property of being different from *B* is not internal to *A* relative to its being white in color.

Mr. Blanshard's hypothetical reply to the objection does not therefore dispose of it, unless indeed he construes the nature of a thing to be identical with the total set of attributes and relational properties the thing possesses. In that case, however, he has been arguing strenuously for a truism that no one would care to dispute.

(c) It is pertinent to note, moreover, that even if one were to grant Mr. Blanshard's claim that all the characters a thing possesses are internal to it, his major task would still be ahead of him. For he would still have to show that the necessary relations in which individuals stand to one another satisfy his requirements for a perfectly coherent rational system. In particular, there would remain the task of showing that the complex of characters which constitute an individual thing's nature do indeed form such a system—so that if *P* and *Q* are any two characters that are elements in a thing's nature, *P* and *Q* mutually entail one another. On the face of it, this seems like a hopeless undertaking, if modern mathematics and natural science do not deceive us in asserting that there are many characters which are logically independent of one another. And unless Mr. Blanshard can find an answer to what appear to be cogent demonstrations of such independence, he must surrender his conception of what is the ultimate ideal of reason.

4.

This last observation leads directly to the second division of Mr. Blanshard's arguments, which attempts to show that every universal is internally related to every other.

One approaches the discussion of this part of Mr. Blanshard's thesis in the cheerful hope that the obscurities surrounding the claim that concrete things are internally related to one another will no longer plague us. For there is a fairly clear sense in which relations between universals may be said to be internal to their natures. Thus, the relational property of having an area greater

than that of any other closed plane figure with the same perimeter may be said to be internal to a Euclidean circle, because this property is logically entailed by the nature of Euclidean circles. On the other hand, the character of having radii of four feet is not internal to Euclidean circles, because neither this character nor any of its contraries are logically implied by that universal. In this sense of the word "internal" it would appear therefore that some universals are internally related while others are not, so that in consequence the doctrine of internal relations ought to be judged as false.

However, Mr. Blanshard does not permit us to decide the merits of the doctrine so quickly. For he makes plain that he is affirming the validity of the doctrine for what he designates as "concrete universals," not for the "false" or "abstract" universals of which examples have just been given. To be sure, this qualification carries with it at least the tacit acknowledgment that the doctrine is false when abstract universals are taken to fall within its scope, and so much at any rate may perhaps be regarded as settled. And since, as I believe, it is with the interrelations of abstract universals that discursive thought (in the sciences and elsewhere) is primarily concerned, an ideal of reason that is based on the presumed truth of the doctrine that concrete universals are internally related does not appear to be obviously relevant to the normal operations of reflective inquiry. But I must also confess that I am quite unclear as to what one is to understand by the phrase "concrete universals," if the expression does not signify *concrete individuals* in all their manifold relations and dependencies; and if this is at least approximately the meaning of the phrase, all the obscurities which attend the doctrine of internal relations when applied to individuals make their unwelcome reappearance when the doctrine is applied to concrete universals.

However, this may be, Mr. Blanshard defends his claim that all universals are internally related, chiefly by trying to dispose of a number of standard objections to this thesis. His replies to these objections bring to a focus several crucial questions, and I shall therefore examine two series of representative comments he makes that bear upon them.

(1) A typical criticism of the doctrine, formulated by Mr. Blanshard runs as follows: "Certain abstractions in the field of

quantity, for example the number three, remain the same and unaffected through every possible embodiment, and in every possible context."[12] But since the embodiment of such universals does not necessarily involve the embodiment of other abstract characters, the former are not internally related to the latter. Hence not all universals are internally related.

Mr. Blanshard counters this criticism with a threefold rejoinder.

(a) His first comment is that the alleged independence of the number three from context is not an independence in all respects, "for it is so intimately bound up with the other members of the number series that if its relations to any one of them were altered, if three were no longer greater than two, for example, or less than four, it would simply vanish."[13] I am afraid, however, that Mr. Blanshard is here scoring only against mythical opponents and against those who confuse the contradictory of a proposition with its contrary. His present remark carries no weight against those of his critics who, in denying that *all* relations of the number three are internal to its nature, do *not* deny that *some* of its relations are internal.

(b) The second part of Mr. Blanshard's rejoinder asserts that "identity in difference" creeps into even purely arithmetical analysis. According to him, the equation "$3 = 2 + 1$" asserts that "in some respect or other the two sides are the same." But if the two sides are "merely and abstractly the same, i.e., the same with no difference at all," a distinction is asserted without difference; and if they are "merely different," the equation asserts what is not, for the equation "expressly declares that they are *not* different wholly."[14]

I have not been able to discover what direct bearing these remarks have on the question whether all the relations of the number three are internal to it. The remarks do reveal, however, a common mistake in analysis, the mistake of confusing a sign with what the sign expresses. The essential point to note is that the *equation* "$3 = 2 + 1$" is a complex linguistic sign, whose two members are different *symbolic expressions.* What the equation asserts (assuming that it is not being used to state the nominal definition of

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12. *Ibid*, p. 471.
13. *Ibid.*, p. 472.
14. *Ibid.*

the *numeral* "3"), is that the number referred to by its left-hand member is identically the same as the number *described* by the right-hand member, where the descriptive phrase describes the number in terms of a certain operation upon two other numbers. The identity in difference which Mr. Blanshard finds in the equation thus reduces itself to the following: the same *number* is denoted by two different *expressions*. But surely this fact cannot be used to cast doubt on the claims of Mr. Blanshard's critics that a universal may appear in two different contexts without undergoing any alteration in its nature.

(c) The remaining part of Mr. Blanshard's rejoinder asserts that the alleged indifference of abstracted quantities to concrete contexts is simply the consequence of a definition and cannot therefore be taken as decisive evidence against his view. He thus declares that

> when it is asked whether . . . purely numerical differences, or the assemblies of them of which the several numbers are composed, depend on the special differences of the terms, the answer presumably is No. But does this prove that there are purely numerical differences in nature? It is hard to see that it does. All that it shows is that if one *defines* one's units as independent of special differences, then they will be independent of special differences. It does not show that one's definition corresponds to anything in reality[15]

This curious comment seems to me a child of desperation. Mr. Blanshard is apparently not denying that the number three is a universal. But if the number is a universal, and if its presumed *logical independence* from various other universals with which it may sometimes be conjoined is simply the consequence of its definition, just how, one would like to know, is the number to be conceived so that this logical independence is irrelevant for understanding the true nature of three? Moreover, what good reasons are there for doubting that the number three as defined corresponds, or may correspond, to something in reality? When we discover that two sets of elements in nature (say, the individuals gathered to play Beethoven's Opus 70, No. 1, and the principal planets whose orbits are interior to the orbit of Mars) can be matched in a one-to-one fashion, do we not discover a genuine fact in the real—a fact which is expressed by saying that the two

15 *Ibid.*, p 473

sets possess the common cardinal number three? The obvious truth seems to be that the cardinal numbers, like other universals, are properties of groups of elements that are *invariants* under certain transformations and conditions; and although they are properties which can be defined, the fact *that* they are invariants is not simply a matter of definition. Nor does the assumption that cardinal numbers are invariants entail the conclusion that "there are purely numerical differences in nature." On the contrary, they could not very well be invariants unless the groups of things which they characterize were distinguishable in various respects. Accordingly, to say that the cardinals are invariants is simply another way of saying that they are not internally related to every other character with which they may jointly occur.

(2) Mr. Blanshard's rejoinder to one criticism of the theory of internal relations thus seems to me to be somewhat less than conclusive. I now turn to his comments upon a second objection, which maintains that we *can* have adequate knowledge of a universal (say redheadedness) without knowing all its relations to every other universal that might be exemplified by the individuals possessing the first—for example, without knowing all the relations of redheadedness to the mental and bodily traits of redheaded people. The crux of Mr. Blanshard's reply to this is that while we can, and do, have some knowledge of redheadedness without knowing all its relations, we cannot know "red-headedness *fully and as it really is* without such knowledge."[16]

This reply is certainly conclusive if the phrase "to have full knowledge" simply *means* to know *all* the relations of a character; and perhaps at bottom Mr. Blanshard does rest his case on what is essentially a stipulation as to his use of language. Nevertheless, there are some indications that he is aiming at a less arbitrary disposition of a serious criticism of his views. For he declares that the "red-headedness now explicitly presented to thought" is not "all there is to that attribute as it exists in the nature of things," since an idea "always points beyond itself; it always means more than it is; it always refers to more than it includes within the circle of its explicit content."[17] He continues:

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16. *Ibid.*, p. 488.
17. *Ibid.*, p. 489.

Red-headedness is an integral part of an organism, and indeed is so bound up, for example, with the structure of hair-fibres, and this in turn with all manner of constitutional factors determining racial and individual differences that our common notion of it supplies scarcely more than a sign-post to its real or ultimate nature, i.e., to what it is as embedded in its own context.[18]

But do these explanations remove the force of the criticism? I think not, and for the following reasons:

(a) In the first place, the point of the criticism (namely, that one could have adequate knowledge of redheadedness without knowing all its relations) does not reside in the claim that the redheadedness explicitly presented to thought is "all there is" to this character; at any rate, there are many who raise that objection to Mr. Blanshard's thesis and at the same time deny such a claim. The point of the criticism is that the adequacy of one's knowledge of redheadedness is to be measured in terms of its relevance to the specific problems which may generate inquiry into that character. There are, however, *many distinct* problems which may generate such inquiry, and not just one all-encompassing difficulty; and there is no good reason to suppose that what may be an adequate resolution of one problem is either adequate or relevant to every other. The problem a readheaded woman faces who wishes to adorn herself attractively is not the problem which may agitate the physiologist or geneticist, and neither of these problems coincides with the question that a student of the physics of color may put to himself. Why should one imagine that these various problems are simply limited aspects of one inclusive problem, or that the several answers to them are necessarily relevant to one another? And why should one suppose, *in advance of specific inquiry*, that in the attempt to answer any one question about redheadedness one is inevitably and necessarily led to the consideration of *every* relation in which that character stands to others?

(b) I come to my second reason. A customary way of distinguishing between universals and concrete individuals is to say that the former, unlike the latter, are capable of repeated exemplification and are often definable. But according to Mr. Blanshard, redheadedness is no more repeatable and definable than are the

18. *Ibid*, p. 490.

individuals who may happen to illustrate it. For if I read him aright, the redheadedness embodied in Frederick Barbarossa is regarded by him to have a "real or ultimate nature" which is different from the nature of redheadedness embodied in one of Barbarossa's ancestors. And if this is so, in what sense is the redheadedness Mr. Blanshard is discussing a universal, and in what way are his remarks relevant to the criticism he is nominally discussing?

(c) I have one final point in this connection. Mr. Blanshard is presumably considering the question whether all of the relations which redheadedness may have to other characters are internal to redheadedness. His aim must therefore be to determine whether, if an individual *A* is redheaded, it *logically follows* that *A* possesses every one of the traits it does in fact possess—for example, that *A* is blue-eyed, brachycephalic, right-handed, and so on. But what he is actually discussing is the question whether these other traits are *causally related* to *A*'s hair being reddish in color. Now while it may indeed be the case that the occurrence of redheadedness has causal conditions and consequences, it is a complete *non sequitur* to conclude from this fact that the characters causally connected with redheadedness are *internally* related to it—it is a conclusion which is warranted only if it can be shown, what thus far Mr. Blanshard has not shown, that logical entailment is an essential ingredient in all causal relations.

I must therefore conclude that Mr. Blanshard does not establish his claim that the relations of universals are all internal, whether the universals are taken to be concrete or abstract. In particular, he presents no plausible reasons for doubting that the demonstrations contained in the modern literature of logic and mathematics concerning the logical independence of various universals do prove what they say they do. The challenge that these demonstrations offer to the doctrine of internal relations is certainly not a negligible one; and one of the strange anomalies of his defense of the doctrine is that he addresses himself to it only incidentally.

5.

I have now examined two of Mr. Blanshard's three classes of arguments for the doctrine of internal relations. There remains for consideration his third group, which attempts to find support

for the doctrine in the alleged nature of causal relations. Two important claims are made by him in this connection. The first is that "all things are *causally* related, directly or indirectly"; and the second is that "being causally related involves being *logically* related."[19] I shall, however, not stop to examine the evidence Mr. Blanshard offers for the first claim, chiefly because if, as I hope to show, his grounds for the second are insufficient to establish it, his first claim even if sound would not by itself suffice to prove the doctrine of internal relations. Certainly many thinkers have held that all things are causally related but have rejected the doctrine without demonstrated inconsistency.

Three lines of evidence are presented by Mr. Blanshard to show that causal connections involve logical necessity.

(1) In the first he maintains that whenever we engage in deductive inference, "the fact that the ground entails the consequent is one of the conditions determining the appearance of this consequent rather than something else in the thinker's mind."[20] Accordingly, the answer to the question "Why does the conclusion of an argument appear in the mind of a reasoner?" is that the *thought* of the premise, which constitutes the cause (or part of the cause) for the occurrence of the *thought* of the conclusion, *logically necessitates* this latter thought. There is therefore an element of logical necessity relating the cause and the effect.

Mr. Blanshard appears to take much stock in this argument, for he has used it on more than one occasion to win assent for his views. Nevertheless, I find it singularly unimpressive.

(a) It is not an unfamiliar fact that at least in some cases when a man thinks of a premise he subsequently thinks of a proposition which, though he believes it to be the logical consequence of the premise, is in fact not a valid consequence at all. If we admit that in such cases the thought of the premise is a cause (or part of the cause) of the thought of the conclusion, we must also admit that thoughts may be causally related, though the propositions to which these thoughts are addressed do not stand to each other in the relation of logical entailment. There is therefore some ground for believing that the presence of the implicative relation between

19. *Ibid.*, p. 492.
20. *Ibid.*, p. 496.

propositions is not a *sine qua non* for the alleged causal connection between *thoughts* about those propositions.

It is also well known that men often entertain propositions with a view to deducing conclusions from them but nevertheless fail to do so, even though various conclusions may in fact be entailed by the premises. Evidently the presence of the implicative relation between propositions, therefore, is not a *sufficient* condition for the causal determination of a thought about a conclusion by a thought about the premises.

It sometimes happens, moreover, that each of two men will think of a premise and also come to think of a conclusion implied by it, where one of the thinkers perceives the logical connection between the propositions while the other, luckily hitting upon the conclusion, does not obtain it by following the chain of logical implication. Such a situation is almost ideal for the application of the familiar canons of induction; and if we rely on the Method of Difference, we must conclude that though the thought of one proposition may be the cause (or part of the cause) of the thought of a second implied by the first, the relation of implication is not an element in the causal transaction. Contrary to Mr. Blanshard's contention, his argument thus supplies no credible reasons for supposing that causal connections involve logical necessity.

(b) There is, however, an even more serious flaw in his argument. What is it, we must ask, which is properly characterized as "necessary" when what is called a "necessary inference" is drawn? When, for example, we draw the conclusion that Smith is younger than Brown from the premise that Brown is older than Smith, is it the *inference* which is necessary, or is it the *proposition* that if Brown is older than Smith then Smith is younger than Brown? The answer is clearly in favor of the second alternative. For it is of the *proposition*, not of the inference, that it is correct to say: it is necessary because it is *impossible* for its antecedent to be true and its consequent false; it is not at all impossible for an *inference* to occur whose antecedent is true and its consequent false. In characterizing an inference as necessary we are thus using an elliptic form of speech, and the phrase "necessary inference" must be construed as signifying the fact that the consequent of a necessary conditional proposition is being deduced from its antecedent.

Accordingly, to argue that the causal relation between the

thought of a premise and the thought of the conclusion (when the premise entails the conclusion) involves logical necessity is to confuse the thought of a necessary relation with the necessity of a thought, it is to confound the nontemporal logical relation of entailment or implication with the temporal process of inference that recognizes or discovers such implicative relations.

Mr. Blanshard is not unaware of this apparently fatal objection to his argument. But his reply to it is regrettably not to the point and succeeds only in raising irrelevant issues. His rejoinder considers the objection as if the latter rested on the assumption that causal connections hold between "mere event[s], endowed with no sort of character",[21] and in opposition to this assumption he maintains, quite rightly, that the "contents or characters of events" enter into causal processes. He therefore concludes that the logical relations between these characters also enter into these processes and declares: "In explicit inference we have a process in which we can directly see not only *that* one event succeeds another, but in large measure *why* it succeeds."[22]

But just what is the pertinence of these remarks to the matter at stake? For suppose we admit that the thoughts which are said to be causally related in inference are not naked events, stripped of all characters. It does not follow from this admission alone that the *logical relations* between the *objects* of those thoughts enter into the causal processes involving those *thoughts;* and it certainly does not follow from that admission that it is the *thoughts as existents,* rather than the *propositions* to which those thoughts may be addressed, which logically imply one another. Mr. Blanshard cannot be acquitted of the charge that he is confusing implication with inference.

Moreover, is it the case that we do directly see, as Mr. Blanshard maintains, *why* in an explicit inference one event follows another? Do we see *why,* when we think of Brown as older than Smith, we subsequently think that Smith is younger than Brown? It has already been noted that though the first proposition entails the second, the *thought* of the first is not invariably followed by the *thought* of the second; and it is not unreasonable to suppose therefore that the causal sequence of such thoughts involves the

21. *Ibid*, p 497.
22. *Ibid.*, p. 498.

operation of a complicated physiological and psychological mechanism, whose detailed structure and conditions of effective performance are still only partly understood. Accordingly, there seems to be some basis for the suspicion that when Mr. Blanshard believes he sees *why* one event in inference is followed by another, and not merely *that* there is such a succession, he is being deceived by the happy working of his own body into identifying his *apprehension* of necessary relations with an alleged necessity of his apprehending those relations.

(c) There is one other aspect of Mr. Blanshard's discussion that requires brief comment. For he believes that serious consequences for morals and the life of reason follow from the denial that logical necessity is involved in causal relations; and he declares that "unless necessity does play a part in the movement of inference, no argument will establish anything,"[23] since on the hypothesis that no such necessity exists the distinction between being "moved by reasons" and being moved by causes is simply an illusion.

Now, no doubt, all who love the life of the intellect and hate brutal unreason will recoil in horror from any philosophy which would deny this distinction. But can one retain it only on Mr. Blanshard's terms and only within the framework of his philosophy? Surely not. Why is it impossible to be moved by reasons if the *temporal passage* from premise to conclusion in a valid inference does not involve a relation of logical necessity? A man who first notes a premise *A*, and then perceives that *A* logically implies *B*, *is* moved by reasons when he accepts *B* on the evidence of the premise—even if the causal sequence, the thought of *A*, the perception of the connection between *A* and *B*, the assertion of *B*, is a logically contingent one. Such a thinker might not assert *B* did he not *perceive* the connection between *A* and *B;* and his *perception* of this connection is doubtless one of the factors which causally determine his thought and acceptance of *B*. But is there any reason for maintaining that if the connection between this factor and the effect attributed to it is a logically contingent one, its manifest operation is illusory?

(2) So much for Mr. Blanshard's first argument for the presence of logical necessity in causal relations. He next comes to alleged cases of such necessity in mental activities other than inference.

23. *Ibid.*

And he offers as an example of such necessity the proposition that all who think lightly of their own deserts are grateful, in which, according to himself, both a causal and a logical connection is asserted between low self-esteem and gratefulness for the esteem of others.[24]

As far as I can make out, Mr. Blanshard rests his case that this is so on the alleged fact that though "one cannot isolate in human nature the precise reciprocating conditions of gratitude, or formulate one's law in anything better than a statement of tendency," nevertheless "we do have *some* insight into why the man of low self-esteem should be grateful for the esteem of others."[25] He therefore cites with approval Ewing's assertion:

> It seems to me that we can see and to some extent really understand why an insult should tend to give rise to anger, why love should lead to grief if the object of one's love die or prove thoroughly unworthy, why a success should give pleasure, why the anticipation of physical pain should arouse fear. It does seem more reasonable *on other than inductive* grounds to suppose that if *A* loves *B* that will tend to make him sorry when *B* dies than to suppose that it will make him intensely glad.

I will not venture to challenge Mr. Blanshard's contention that in such matters as he mentions he *does* possess an "insight" into the presence of a necessary logical bond, especially since he specifies no general rules that might serve to define the character of that necessity. If he does have the insight, he must be congratulated on possessing what is surely a rare power. However, Mr. Blanshard himself admits that the alleged law connecting low self-esteem and gratitude states only a "tendency," not an invariable connection, to which therefore exceptions may (and presumably do) occur. And I confess that the sense in which a law expressing only such a tendency *also* expresses a logical necessity is to me entirely obscure.

Moreover, it is surely no news that many men with a low self-esteem exhibit an attitude quite the reverse of gratitude for the esteem of others. Spinoza had suggested as much, and in the light of contemporary psychological investigations the absence of feelings of gratitude in such cases appears eminently plausible. The chief point to note, however, is that whether a certain type of

24. *Ibid.*, p. 500.
25. *Ibid.*

human response to an indicated situation appears "reasonable" and "logically necessary" or not, is a function of what theory of human nature is explicitly or implicitly assumed. But one must not overlook the crucial fact that though many propositions about human action may be necessary consequences from the main principles of the theory, neither those principles nor those propositions are logically necessary truths. For example, Mr. Ewing's example of love for a person leading to grief if that person dies is a theorem in Spinoza's *Ethics;* but its "necessity" is relative to the postulates of this system, postulates which, if they are true, are clearly only contingently true.

Apropos of the suggestion that certain general propositions about human actions are "reasonable" inherently and "on other than inductive grounds," I must add the obvious but unfortunately still needed reminder that the pages of the history of thought are strewn with exploded claims concerning the "necessary" character of various "truths" alleged to be revealed to immediate vision. The tendency to see something final and necessary in what subsequently turns out to be transitory and contingent has been no minor hindrance to the development of knowledge, especially in the social and moral disciplines. To be sure, Mr. Blanshard's claim to have discovered such logically necessary propositions about human actions may meet a better fate than have similar claims by countless other men. It is nevertheless curious that such a claim should come from one who, in terms of his professed philosophy, might be expected to deny that necessity and self-evidence characterize propositions isolated from their relations to some system in which they are elements.

(3) I turn finally to Mr. Blanshard's discussion of the question whether logical necessity is present even in the causal processes found in physical nature, which clearly constitutes what is perhaps the most crucial part of the defense of his general thesis. However, his discussion is predicated on the assumption that only two views as to the nature of causal connections are possible, one represented by what is known as the regularity view, the other by the conception which he himself favors. He therefore devotes his best efforts to a criticism of the regularity view, in the apparent belief that if he can exhibit its inadequacy he will thereby have established the validity of his own conception. Unfortunately for the

argument, the two alternative analyses Mr. Blanshard considers do not exhaust the possibilities; in fact, a number of contemporary writers (for example, Cohen, Dewey, Parker, and at one time Broad), have offered accounts of causality which are incompatible both with the regularity and the entailment views. Accordingly, even if Mr. Blanshard's reasons for rejecting the regularity view were entirely cogent, he would still not have produced compelling evidence for adopting his own analysis of causality.

In what follows I shall therefore not consider his criticisms of the regularity view, and shall restrict myself to examining the few grounds he presents for the entailment view. These grounds are, I think, just two in number.

(a) Mr. Blanshard requires of any analysis of causality that it be compatible with the fact that successful predictions concerning the future can often be made on the basis of past observations on the sequences of events. And he maintains that when we predict that *b* will follow *a* in the future as it has followed in the past, there must be a logical bond between *a* and *b* which warrants the prediction. "Unless *a* is connected with *b* by something more than mere conjunction," he declares, "there is no ground . . . whatever" for the argument from past to future.[26]

It will be admitted, I think, that if all causes *entail* their effects, and if we knew not only this but *also* that a specific phenomenon *a* which is suspected of being the cause of *b* entails the latter, then a prediction concerning the future occurrence of *b* on the strength of observing *a* would be fully warranted. However, if we knew only that the entailment view is true but did *not* know that *a* logically implies *b* (though events of the type *a* may have been observed in the past to be followed by the events of type *b*), we would certainly not possess what Mr. Blanshard would regard as rationally satisfactory grounds for predicting the future occurrence of *b* as an effect of *a*. Evidently, therefore, the acceptance of the entailment view of causality is not sufficient for justifying any particular prediction.

But though some defenders of the entailment view claim to have an "insight" into the logical structures of specific causal processes in physical nature, Mr. Blanshard makes no such pretensions; he modestly limits his own claims to matters pertaining to mental

26. *Ibid.*, p. 507.

actions. And there is little doubt that most men who venture to predict physical occurrences also lack such insight. What then can the entailment view of causality, assuming that it is the correct view, offer to Mr. Blanshard and the rest of mankind in the way of a "rational justification" of predictive inferences? Must not he, like everybody else, fall back upon the evidence provided by past conjunctions of characteristics to support the hypothesis that they may be causally related? Is he any better off in this respect than are those who subscribe to the regularity view of causality? Must we not conclude that the entailment view contributes *nothing* toward advancing the aims of specific inquiries into the causal dependencies of physical nature, that it provides no rational foundation for the successful predictions that are often made, and that therefore Mr. Blanshard's present line of reasoning supplies no support for the entailment view?

(b) Mr. Blanshard's remaining argument for the entailment view rests upon a consideration of general statements *about* causality (such as the maxim "Same cause, same effect"), rather than upon a study of specific causal propositions (such as that the earth's rotation is the cause of day and night). He maintains that we can safely assert such general propositions about causality because we possess an "insight" that justifies our doing so. This insight consists in recognizing that "when *a* is said to produce *x* in virtue of its nature as *a*, the connection referred to is not only an intrinsic relation but a necessary relation."[27] And as he goes on to explain, "To say that *a* produces *x* in virtue of being *a* and yet that, given *a*, *x* might not follow, is inconsistent with the laws of identity and contradiction." For *a* is not a mere "cluster of qualities abstracted from their relations"; on the contrary,

> *a*'s behavior is the outgrowth or expression of *a*'s nature. And to assert that *a*'s behavior, so conceived, could be different while *a* was the same would be to assert that something both did and did not issue from the nature of *a*. And that is self-contradiction. The statement would also . . . conflict with the law of identity. It implies that a thing may remain itself when you have stripped from it everything which it is *such as* to be and do. To strip it of these things would be to strip it, so to speak, of the suchness that makes it what it is, i.e., to say that it is other than it is.[28]

27. *Ibid.*, p. 512.
28. *Ibid.*, pp. 513-514.

Old acquaintances thus greet us once more, the puzzle as to what is to be included in a thing and the obscure notion of a thing's nature. Let us make one final effort to penetrate into these mysteries, by applying Mr. Blanshard's present argument to the proposition that Brutus caused the death of Caesar. On that argument, Brutus' action was the outgrowth of Brutus' special nature, and to suppose that Brutus had not acted as he in fact did would be to strip him of the "suchness" that made him what he was. The argument thus requires us to say that the compound proposition, Brutus did cause the death of Caesar but it is nevertheless logically possible for Brutus not to have done so, is logically impossible More briefly and generally, Mr. Blanshard's position as revealed by the present argument reduces to this: every true proposition which imputes a causal action to Brutus is logically necessary.

This is certainly an amazing conclusion. But is it true? It can be shown to be true if, and only if, the *individual* Brutus is conceived as including every possible attribute that may be truly predicated of him, while at the same time the *nature* of Brutus is equated with the total set of characters Brutus is thus made to include. In short, it can be shown to be true only by a violent redefinition of the expressions "individual" and "the nature of an individual." But as I have tried to show earlier, by this device Mr. Blanshard's entire thesis is reduced to a trivial tautology.

I therefore conclude that Mr. Blanshard's heroic efforts in behalf of the doctrine of internal relations have failed of their intended objective. He has not succeeded in showing that contingency is not an irreducible feature of the world, and in urging upon men an ideal of reason which ignores this character of things he must be judged as an advocate of a false and irrelevant ideal. The vision he has called up of the scope and office of human reason is not without grandeur and inspiring power, and its insistence on system and rational order reveals its sources in human aspirations. But like all visions which feed on uncontrolled and exaggerated hopes and fancies, it is a vision that cannot permanently serve to guide the energies of sober men.

16

The Perspectives of Science and The Prospects of Men

THERE ARE TWO MODES in which human life is influenced by new developments in theoretical science. Since the beginnings of systematic inquiry in classical antiquity, advances in fundamental knowledge have prepared the way for a more effective practical mastery of the environment; and the technological innovations to which they frequently give rise—in agriculture, industry, medicine, or warfare—have produced radical transformations in traditional patterns of social living. This aspect of science is now a commonplace in the writings of modern historians and students of human affairs. It is certainly not neglected in recent popular literature on the wonders wrought by present-day inventions. Indeed, many distinguished scientists with an attentive eye on the ultimate source of financial support for pure research are now stressing the practical fruits often gathered from investigations which initially do not

promise such a harvest, and the utilitarian values of science undoubtedly loom large in the minds of those who today eventually foot the staggering costs of modern research. However, there is a second and less publicized way in which scientific developments affect men's lives: by challenging established beliefs about the cosmos and its parts, and inducing emendations in habitual modes of thought. Revisions of ancient creeds and alterations in intellectual habits are not exclusively the products of major theoretical revolutions like those of Newton and Darwin, they may also be called forth by relatively minor additions to knowledge like those associated with voyages of exploration and the study of ancient or primitive cultures. The flood of books and articles in our own day on the bearing of current scientific discoveries upon inherited conceptions of nature and man is visible testimony to this aspect of the impact of science on human life.

Although there are different channels through which scientific developments exert their influence, changes in knowledge eventually lead to a re-examination of the ideals which express man's basic aspirations, of the principles by which men evaluate their actions, and of the methods which men employ in deciding between alternative moral claims. Technological innovations of any magnitude often make it difficult, if not impossible, to continue customary modes of conduct; and they may produce fashions of living that are not congruous with, or are not envisaged by, traditional moral ideals. Moreover, though new theoretical and factual discoveries do not always introduce any overt changes in social practice, they may nevertheless become highly pertinent in the evaluation of social policy and in the justification of moral attitudes. An ideal of human life which seems eminently reasonable on one set of factual assumptions may be utterly without cogency when viewed in the perspective of altered scientific conclusions. Accordingly, an integrated system of moral commitments can be disrupted by developments in science, and, individually as well as collectively, men are sometimes made distraught by felt incompatibilities between traditional moral standards and new advances in knowledge. Reflective men are thus perennially confronted with a threefold task of criticism: of clarifying the bearing of trends in scientific inquiry upon pervasive conceptions of man's place in nature; of making explicit the intellectual methods by which responsibly held beliefs are achieved;

and of interpreting inherited beliefs and institutions in the light of current additions to knowledge—all this, in order to exhibit the enduring wisdom which may be embodied in them.

It has been the traditional office of philosophy, whether lay or professional, to contribute to this difficult task. Professional philosophers frequently engage in heated debate concerning the subject matter and the problems that are proper to their discipline. Although their conclusions are often in sharp disagreement with each other, the record of history leaves little room for doubt as to what in the main their ultimate concern has been. Indeed, even the recurrent disputes over the legitimate scope and method of philosophy illustrate the conception of philosophy as a critical commentary on science, and as a continuing interpretation of human experience in the perspective of fresh scientific achievements. It is certainly a striking fact that some of the most brilliant periods in the history of Western philosophy coincide with eras during which frontiers of knowledge were being pushed back vigorously. It is hardly an accident that the flowering of philosophic thought in Greek antiquity came on the heels of fundamental discoveries in mathematics and biology; that the influential speculative and critical philosophies of the seventeenth and eighteenth centuries were produced during the decades which saw the rise and consolidation of modern physical science; or that the ambitious philosophies of evolution in the nineteenth century were developed at a time when knowledge of the human past as well as of the history of other forms of organic life was being rapidly expanded. The adequacy of these various philosophies as systems of warranted truth is a matter of controversy, upon which general agreement is hardly likely in the foreseeable future. These systems, nevertheless, exhibit the historical role of philosophy as the examination of the significance of science for human weal and woe; and this role is easily discerned in the literature of contemporary philosophy as well. In a readily identifiable sense, all of philosophy has been, and continues to be, a critique of science. And while what is today commonly designated as the philosophy of science is the investigation of a loosely defined set of special problems, the boundaries of the philosophy of science are in fact the boundaries of philosophy itself.

For several decades, however, in Western Europe and America, philosophical reflection on science has been largely directed to logi-

cal and methodological issues generated both by the remarkable theoretical innovations of physical science and by new approaches in the study of human society. These developments have not only upset deeply rooted certainties concerning the structure of the physical universe and the behavior of the human individual; they have also compelled a re-examination of supposedly firm criteria of intelligibility and the grounds of cognitive certitude. Euclidean geometry has been displaced from its age-old status as the only conceivable foundation for a comprehensive theory of nature. A system of chronometry has been introduced which is sharply at variance with customary notions of temporal order. Apparently cogent reasons have been found for curtailing the scope of regulative and constitutive principles, like those of causality and continuity, which have long been regarded as paradigms of absolute universality and necessity. Evidence has accumulated for conceptions concerning the springs of human action which are deeply disturbing to widely held assumptions about the basis of human rationality and responsible action. And comprehensive theoretical explanations of newly discovered phenomena have become current which, despite their enormous success in accounting for detailed facts, postulate an executive order of nature that seems alien to human experience and appears paradoxical even to expertly skilled scientists.

It is not altogether surprising that intellectual changes of the magnitude here only briefly indicated have found interpreters who see in the latest findings of science renewed grounds for comforting superstitutions, or who defend in the very name of science itself creeds to which the use of a disciplined but free intelligence is basically hateful. If such interpretations seem plausible to many, this is partly because scientists have in the main failed to clarify for themselves as well as for others the actual content of their new theories and the rationale of their intellectual methods. Indeed, distinguished workers in the sciences have themselves sometimes supplied commentaries on current reorientations in scientific theory that are at best examples of uncontrolled fancy, and are frequently exercises in obscurantism. This situation is not quite as anomalous as it may appear at first glance. To be sure, skill and understanding are at their best, as Santayana noted, when they adorn the same

mind. Nor is there much doubt that perhaps the most illuminating analyses of scientific achievements and procedure have been made by philosophical-minded practicing scientists. What is loosely called "scientific method," however, is generally a habit of workmanship that skilled investigators possess, and not a codified set of principles which they explicitly acknowledge. Those who are successfully engaged in specialized research usually show no serious interest in methodological analysis which does not contribute directly to the solution of specific research tasks. Their philosophy of science is often hardly more than the echo of philosophic ideas uncritically acquired during their early schooling. There is little agreement among scientific workers on the broad significance of their theories or on the logic of their procedure, despite their remarkable virtuosity in handling complex intellectual tools, and in striking contrast to the eventual unanimity which they achieve on solutions for specific technical problems. There are, indeed, no uniform standards of competent workmanship, whether among scientists or professional philosophers, which control the analysis of such matters. There is still lacking a generally adequate technique for unravelling the enormously complex maze of symbolism involved in the construction and the use of scientific theory.

The symbolic nature of scientific theory is certainly not a recent discovery. Nevertheless, the subtle and indirect ways in which theories function as schemata of representation have not been widely noted or appreciated until fairly recently. A century ago, it was common to think of a theory—even a theory in physics—as an inductive generalization derived by abstraction from directly observed occurrences. A theory was assumed to be simply a compact description of regularities that obtain between phenomena. The successful development of theories which ostensibly postulate inherently unobservable entities and processes made evident the superficiality of this account. For a time the view then became prevalent that postulations of "unobservables" are nothing but convenient though fictional modes of speech. This amended version of the "simple description" notion of theory became incredible, however, when impressive experimental evidence was found for the "reality" of molecules, atoms, electrons, and the like. Many thinkers have therefore concluded that the fundamental theories of

modern physics and chemistry are about an order of things and events that are existentially and causally prior to the things and events encountered in daily life.

The unobservable but basic entities of nature, however, ostensibly possess few if any of the traits that characterize the objects of familiar experience. Moreover, they seem in many respects to be incommensurably unlike the latter. The relation of the "world" of gross experience to the "world" disclosed by physics thus became an acute problem. Some writers resolved it by relegating one or the other of these "worlds" to a realm of metaphysical appearance. Others sought to overcome it by construing the postulated scientific objects in terms of the categories of human experience, and thereby clothing the unobservable entities of physics with properties analogous to those distinctive of the human organism. Still others solved it by postulating a progressive evolution or emergence of levels of being. But the proposed solutions raise more issues than they settle, and they are all based on the dubious assumption that each constituent part of a theory is the intellectual image of an identifiable item in the subject matter of inqury. On this assumption a theory is therefore a description of some fixed realm of ultimate fact. But the assumption simply ignores the complex structure of theoretical symbolism. Those who make this assumption fail to appreciate the flexible uses of theoretical expressions, the variety of special meanings which are associated with them in different contexts of inquiry, the multiple regulative roles which theories play, or the different logical functions which grammatically similar statements may possess. These matters are more fully understood at present. Contemporary philosophy of science has succeeded in dispelling much of the mystery that is produced when the elliptic formulations of mathematical physics are construed in a myopically literal manner.

Indeed, much of the recent literature of the philosophy of science is a self-conscious attempt at articulating modes of symbolic signification, and a search for effective criteria of meaningful discourse. This search is often controlled by the therapeutic objective of eliminating pseudo-problems both within science proper and in the philosophic interpretations of science. In the pursuit of this objective, variants of Charles Peirce's "pragmatic maxim" for making our ideas clear have played a prominent role. According to that

maxim, our conception of an object of thought consists entirely of the "practical" bearings which we suppose that object to have. The adoption of the maxim leads, on the one hand, to the rejection as spurious of many allegedly profound puzzles raised by commentators on science, and, on the other hand, to the detailed study of scientific formulations in terms of the concrete procedures and habits of usage that alone invest them with significance. Peirce himself noted, for example, that the term "force" in physics does not represent a "mysterious entity," as was commonly believed by many of his contemporaries. On the contrary, the term is completely understood when its role in the equations of physics is made explicit, and when the use in inquiry of those equations is indicated. It is therefore absurd to claim, as many eminent scientists have done, that while we may understand the effects of force we do not understand what force itself is. Many thinkers in Europe and America, in many instances without having been exposed to the influence of Peirce's writings, have adopted his maxim in a variety of forms. They have extended the type of analysis he proposed for the notion of force to a large number of other ideas in physics, biology, psychology, and social science. In consequence, fundamental contributions have been made not only to the clarification of specific theoretical concepts, but also to the understanding of the structure and operation of scientific symbolism in general.

It must be acknowledged, however, that oversimplified versions of the Peircean maxim have gained wide currency, and that injudicious use of them has frequently darkened counsel. Many contemporary writers have supposed that a criterion of meaning contains in capsule form the solutions for all mortal ills, and in consequence they have made grotesque claims for their approach to the analysis of scientific discourse. Other writers have espoused simple-minded tests of significance, and have shown themselves insensitive to the influence of context upon the meaning and function of statements. They have thereby been compelled to classify as species of "nonsense" nearly all statements of science. But in spite of the quantity of material, incompetent, trivial, and irrelevant, that has been published in consequence, there has been substantial achievement as well. In the hands of analysts competently familiar with the actual operations of scientific inquiry and with the role played in it by theoretical constructions, the Peircean maxim in some of

its variant forms has undoubtedly been an instrument of genuine illumination.

It is difficult to exaggerate in this connection the far-reaching and salutary influence of the theory of relativity upon current philosophy of science. For the Einsteinian critique of classical mechanics called attention to the importance of construing scientific notions by way of the actual operations which control their range of application; and it made impressively clear that apparently significant statements often possess no physical content, precisely because crucial terms occurring in them are associated with no specified procedures of application. Einstein's analysis also made evident that the construction of a theory involves, among other things, a series of decisions between alternate modes of representing and organizing relevant features of a subject matter—for example, in the case of mechanics, decisions between alternate geometries and alternate chronometries. Such decisions are logically arbitrary, since they are not implied by experimental data; they can nevertheless be evaluated in the light of their effectiveness in systematizing fields of inquiry. Furthermore, the theory of relativity reveals unmistakably that when the range of application of a scientific symbol is enlarged, the symbol may undergo fundamental changes in meaning. This has been the history of such terms as "mass" and "energy" in physics, "species" and "image" in biology and psychology, and "class" and "property" in social science. Subtle analogies may control the process of enlarging the scope of application of a symbol. Nevertheless, it is in general a blunder to interpret the more inclusive use of an expression in terms of its initially more restricted meaning. It is this blunder which underlies much of the alleged "unintelligibility" commonly attributed to modern physics. These methodological observations are, however, pertinent for the understanding not merely of a special physical theory but of all theoretical constructions. They have rendered valuable service to thinkers seeking to develop a generalized account of knowledge that is founded on the realities of scientific procedure and on the actual character of scientific explanation.

The classical conception of scientific knowledge was formed under the influence of demonstrative geometry as traditionally taught, and rests on three fundamental assumptions: Genuine scientific knowledge is demonstrative knowledge, and science seeks to

"save the phenomena" by exhibiting the events and regularities of nature as consequences of universal truths. Secondly, since every demonstration must start from premises which are not themselves demonstrable, there must be transparently luminous universal truths which the intellect can grasp as self-evident. Thirdly, if the particulars of sense are to be truly explained, the basic premises of a science must be necessary truths, which are better known and more certain than anything explained by them. These assumptions have dominated scientific, philosophic, and popular thought since antiquity. For example, it was a widespread supposition a half-century ago that the axioms of Newtonian mechanics conform to the classical requirements for the first principles of a science. The discovery soon thereafter that the Newtonian system is not fully adequate to the facts therefore appeared to a number of commentators as a symptom of "the bankruptcy of science."

Many thinkers continue to demand of genuine knowledge the characteristics postulated by the classical ideal of science. The first requirement in that ideal undoubtedly does represent a permanent though partial objective of scientific research. But the remaining components in that ideal are not, and cannot be, achieved by the actual methods of any positive inquiry. Contemporary analyses of the kind previously mentioned have shown, for example, that a theory is never uniquely determined by any set of empirical data, however numerous and varied these may be. Alternate explanations are always possible in principle; and the supposition that a unique explanatory principle is embedded in the phenomena, and shines forth to the attentive intellect, is therefore radically mistaken. It follows that scientific research is not a passive beholding and codifying of self-evident structures in things. On the contrary, the construction of theories, like the construction of works of art, makes serious demands upon powers of imagination and invention. Scientists have repeatedly noted the "free creation of concepts" embodied in their theoretical foundations. As one of them has remarked, the work of Newton, Leverrier, Maxwell, and Hertz "was an expression of their personality just as surely as the work of Giotto, of Shakespeare, or of Bach." This does not mean that the scientist is a demiurge who creates the things he studies. It does mean that an explanatory theory is but one among several possible techniques for representing and analyzing systematically an indefinitely large set

of specific processes. When a scientific theory is constructed in terms of its functions in inquiry, the supposition that the fundamental principles of a science can be established by appeals to self-evidence is thus not even plausible. Indeed, the traditional conception of the relation between a theory and the facts it explains must be partially reversed. A theory does make "intelligible" the occurrences of nature by indicating their interrelations. Nevertheless, the theory is itself "intelligible" not because of its self-luminous necessity and certainty, but because of the manner in which it analyzes and organizes the concrete facts of experience. In short, abstract theory and matters of observation mutually clarify each other. If it is a task of science to "save the phenomena" by making them intelligible in the light of a theory, it is equally its task to "save the abstracta" by making them intelligible in the light of the phenomena they coordinate. Maxwell's electro-magnetic theory, for example, explains a host of electrical, magnetic, and optical phenomena. But the content of the theory becomes clear only when it is understood how its equations are employed for directing specific inquiries and for interpreting matters of observation.

One important outcome of this altered view of scientific knowledge is a correspondingly altered conception of the grounds of scientific certitude. Neither the first principles of a science nor the formulations of concrete happenings are incorrigible. And if the findings of science are reliable, they are reliable neither because they follow from necessarily true basic premises nor because they are derivable from indubitable data of observation. What does support claims to warranted knowledge is the use of a general method (or logic) for establishing a particular claim. Inquiries into different subject matters employ different special techniques of investigation, but underlying them all is a common pattern of procedure for gathering, interpreting and evaluating evidence. A distinctive feature of this method is its doubly self-corrective character. No conclusion of an inquiry is exempt from the challenge of further inquiry, provoked perhaps by doubts raised because of new theoretical constructions or fresh experimental data. And every schema for evaluating the weight of evidence is subject to revision, if further analysis should reveal that the use of a given schema leads to conclusions which are too frequently undependable. The quest for certainty is perhaps an inherent aspect of the scientific enter-

prise. But current clarifications of the self-corrective method of science have made evident that scientific certitude cannot be equated with infallibility. The reliability of a scientific conclusion, it is now clear, is a function of the thoroughness with which it has been tested by methods that on repeated application yield conclusions in good agreement with observed fact. While any given claim to knowledge may be mistaken, some claims are better warranted than others; but the warrant itself ultimately derives from the character of the general policy which science adopts for evaluating all claims.

Considerations such as these have contributed to the formation of a naturalistic conception of human reason and scientific intelligence. Scientific knowledge does not depend on the possession of an esoteric capacity for grasping the necessary structure of some superior reality, nor does it require modes of warranting beliefs which are discontinuous with operations of thought, identifiable and effective in the ordinary affairs of human life. The achievements of science are the products of a cooperative social enterprise, which has refined and extended skills encountered in the meanest employments of the human intellect. The principles of human reason, far from representing the immutable traits of all possible being, are socially cultivated standards of competent intellectual workmanship. The life of reason as embodied in the community of scientific effort is thus a pattern of life that generates an autonomous yet controlling ideal. That ideal requires disciplined dedication without servitude to any ultimate authority, imposes responsibility for performance upon individual judgment but demands responsiveness to the criticism of others, and calls for adherence to a tradition of workmanship without commitment to any system of dogma. To many commentators, the ideals realized in the enterprise of science are also the ideals which are indispensable to the successful operation of any society of free men. Many thinkers, indeed, like John Dewey in America, have based their hopes for the future of mankind upon the extension of the habits of scientific intelligence to every stratum of communal life and to every form of social organization.

Philosophers often suppose themselves to possess avenues to truth —truth which is not accessible to the empirically controlled analytic methods of the sciences. Certainly no shortage exists today of oracular metaphysical pronouncements on the spiritual foundations

of the universe or on the ultimate nature and destiny of human effort. It is not difficult to perceive the reasons why, in a period of acute social tension such as ours, there should be large audiences for philosophies that thrive on obscurantism and which see the shape of things through a glass clouded by anguish and fear. To those who seek from philosophy assurance that life is worth living or that the universe is mindful of human aspirations, contemporary philosophy of science has on the whole nothing significant to say. On the other hand, it has had an unmistakable influence on the development of new approaches in Anglo-American psychology and social science; and it has made an impress on a substantial majority of Anglo-American philosophers, even when their chief concerns are only peripherally related to the analysis of science. A comparison of current philosophical writing in English-speaking countries with similar publications a half-century ago, reveals a greater responsiveness today to the requirements of clarity and cogency than was customary then. There is now a diminished tendency to fall back on *a priori* methods for settling issues that can be resolved, if they can be resolved at all, only by experimental trial and factual study. There is less reliance today on outmoded conceptions of scientific procedure in discussions on the theory of knowledge. There is a greater awareness at present of the dangers implicit in using debatable or half-digested conclusions of factual inquiry as foundation stones for comprehensive systems of political and ethical theory. And greater caution is now practiced when current findings about physical or biological processes are invoked to decide issues in moral and aesthetic analysis.

These are in the main negative achievements for which contemporary philosophy of science is in part responsible. But it can also be credited with an invaluable positive achievement, quite apart from its detailed contributions to the clarification of scientific procedures. For it has given vigorous support and expression to an attitude, at once critical and experimental, toward the perennial as well as the current issues of human life; and it has thereby been a champion of the central values of liberal civilization. The basis for a general outlook on the place of man in nature is supplied by detailed knowledge of the structure of things supplied by the special sciences—an outlook that contemporary philosophy of science has helped to articulate and defend. In the perspective of that outlook,

the human creature is not an autonomous empire in the vast entanglement of events and forces constituting the human environment. Nevertheless, no antecedent limits can be set to the power of scientific reason to acquire theoretical mastery over natural and social processes. Every doctrine which pretends to set such limits contains within itself the seeds of intolerance and repression. Moreover, in the perspective of that scientifically grounded outlook, human aspirations are expressions of impulses and needs which, whether these be native or acquired, constitute the ultimate point of reference for every justifiable moral judgment. The adequacy of such aspirations must therefore be evaluated in terms of the structures of human capacities and the order of human preferences. Accordingly, though the forces of nature may one day extinguish the human scene, those forces do not define valid human ideals, and they do not provide the measure of human achievement. But an indispensable condition for the just definition and the realization of those ideals is the employment and extension of the method of intelligence embodied in the scientific enterprise. A judicious confidence in the power of reason to ennoble the human estate may seem shallow to an age in which, despite the dominant position in it of scientific technology, there is a growing and pervasive distrust of the operations of free intelligence. It may indeed be the case that the temper of mind essential to the exercise of such intelligence has no immediate social future. But the cultivation of that intellectual temper is a fundamental condition for every liberal civilization. By making manifest the nature of scientific reason and the grounds for a continued confidence in it, contemporary philosophy of science has been a servant of men's noblest and most relevant ideals.

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